I.—THE HINDU BEAST FABLE IN THE LIGHT OF RECENT STUDIES.

Part II.

(In this article the following abbreviations are used of Pañcatantra versions: Tantr., the Tantrākhyāyika.—Pa. or Pahl., the Pahlavi translation.—Syr., the Old Syriac.—Bṛh., the (lost) version of the northwestern Bṛhatkathā (archetype of Som. and Kṣem.).—Som., Somadeva.—Kṣem., Kṣemendra.—n-w, the archetype of SP., ν and Hit.—SP., Southern Pañcatantra.—ν, Nepalese Pañcatantra.—Hit., Hitopadeça.—Simpl., textus simplicior.—Pūrṇ., Pūrṇabhadra.)

In the first part of this article (A. J. P. XXXVI. 44 ff.) I described the relationship of the different Pañc. versions in so far as it seems to me determinable at present. Most of the facts as therein stated have been worked out and proved by Hertel. I now face the more unwelcome task of discussing some alleged facts of the same sort which I hold that Hertel has failed to prove. This task, however distasteful, seems to me necessary. For Hertel admits not the slightest question of any part of his "genealogical table" ("Stammbaum") of the Pañc. versions (Pañc., p. 426); he regards every part of it as absolutely and irrefutably proved, and draws sweeping and important conclusions from it. And in fact, as he rightly says (Pañc., p. 428), no real counter-argument has as yet been presented. Various scholars have expressed doubts about the "Stammbaum"; no one has seriously tried to refute any of

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Hertel's arguments in support of it.   It is high time that this be done.   For some of the points in question are of prime importance, for the proper judging not only of the relationship of the Pañc. versions, but of Hindu literary tradition as a whole.

The trouble with Hertel's " Stammbaum " seems to me to be that it tries to prove too much.   Not content with bringing out the many interesting facts to which I have already referred, it undertakes to go farther and establish things which are quite incapable of proof in the present state of our knowledge.   The reputation of all of Hertel's work has suffered, unjustly but quite naturally, on this account.   Some scholars, seeing the subjectivity and unreliability of many of his arguments, have been inclined to assume that all his assertions about the Pañcatantra versions rest on equally uncertain grounds.

I have tried to apply the acid test to all of Hertel's " Stammbaum ", with a view to removing the bad and making the good (which includes the greater part of the " Stammbaum ") all the more certain.   I was somewhat surprised at the ease with which, as it seems to me, the separation can be made. When carefully analyzed, there is amazingly little sound evidence for several of Hertel's allegations—considering the comparative certainty of some of his other conclusions.

The three specific points which I shall try to show are unproved, and probably unprovable, are the following.   I. The archetype "t", containing alleged corruptions found in all existent versions.   II. The archetype "K", from which H. thinks all versions except the Tantr. are descended.   III. The archetype "N–W", from which he thinks Pahl., n–w, and Simpl. are descended.

All of these hypothetical archetypes seem to me mythical. I think there is no closer connexion between Pahl., Som., n–w and Simpl. than between any one of them and the Tantr.   In fact, I have evidence which at least *tends* to show that Tantr. is related to Pahl., and also to n–w and to Simpl., more closely than any two of these latter versions are related to each other. In my careful study of the verses of Book I, I found very few cases of agreement between n–w, Simpl. and Pahl., or any two of them, as against the Tantr., but a considerable number of

agreements between each one of them and Tantr. as against the others.[1]

I. The very first assumption in Hertel's Stammbaum seems to me highly dubious; I mean, his assumption of an early archetype " t ", which stood between the Urpañcatantra (" T ") and all existing versions, and in which Hertel thinks he can point out certain very definite corruptions. He claims to have done this " in für jeden Philologen einwandfreier Weise " (Pañc., p. 443). I cannot let this statement pass unchallenged. Consider what the claim means: it means that Hertel undertakes to establish definite faults in, and to *emend*, a purely hypothetical archetype of *all* existing Pañcatantra versions— an archetype which (together with its uncorrupted original) must have been lost without trace since before the 6th century A. D., according to his theory. "A large order!" Still, I should not wish to deny the possibility of such a thing. But a careful consideration of Hertel's arguments [2] leaves me with the impression that the thing is a myth. Certainly it seems to me that these arguments totally fail to prove his point.

They relate to six passages in which Hertel thinks he can show that all existing versions go back to a corrupt original.[3]

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[1] For instance, as to the mere occurrence or non-occurrence of particular verses in Book I. There are 7 vss. found in Tantr. and n–w only, 32 found only in Tantr., n–w and the Jaina recensions (Simpl., Pūrn.). There are 10 found in Tantr. and Pahl. only, 22 only in Tantr., Pahl. and the Jaina recc. There are 8 found only in Tantr. and Simpl. (or in Tantr., Simpl. and Pūrn.). There are, on the other hand, no verses found exclusively in Pahl. and either or both of the Jaina recensions; none found exclusively in Pahl., n–w and a Jaina recension; not more than 3 (of which 2 are doubtful) found only in Pahl. and n–w; and only 2 found exclusively in n–w and either or both of the Jaina recensions. Do not these facts in themselves make it seem doubtful whether Hertel is right in making Pahl., n–w and Simpl. very intimately related, and only much more remotely connected with Tantr.?

[2] Tantrākhyāyika, (Einleitung zur) Uebersetzung, I. 34 ff.

[3] Incidentally, by what right are all these "corruptions" corrected in Hertel's edition of the Tantrākhyāyika? Since his theory is that the corruptions go back to the *archetype* thereof, why try to make the Tantrākhyāyika text seem better than it really was? Hertel shows no such favor to the Southern Pañcatantra; in his text of it he prints not only all the corruptions of its archetype, but many that were certainly not in it, being found only in manuscripts of the inferior subrecension β.

In case one version or another has the "correct" reading, then he calls it a "glückliche Besserung" (so in his very first instance, to be mentioned in a moment). I am constrained to say that his argumentation on this point seems almost wholly subjective, and of the thinnest sort; its strength is in inverse ratio to the vigor and positiveness with which it is put forward. All six cases concern very trivial matters; generally a slight change in, or omission of, a single word. (Scarcely sufficient evidence, as to mere bulk, to justify such sweeping conclusions!) I cannot here take them all up; two must serve for all. I choose the very first two instances given by Hertel; I presume, from his putting them in that position, that he regards them as at least as strong as any. In this I agree with him; the others are no stronger.

His second instance (which we shall consider first) does not seem, in fact, to show any corruption in the supposed archetype "t" at all, according to Hertel's own statement of the case. His opinion that it does so seems to be merely due to lack of clarity of thought. Hertel maintains that in Tantrākhyāyıka II. 87 the "original" reading was *yasyānuban-dhāt*, which Pūrṇabhadra has, for the Tantr. MSS. *yasyānu-bandhaḥ*; but that the "original" order of the two half-verses was that of the Tantr. (they are transposed in Pūr-ṇabhadra).[1] In other words, of these two points on which Tantr. and Pūrṇ. differ in the reading of this verse, H. thinks that the Tantr. preserves the original reading in one, Pūrṇ. in the other. It appears that if Hertel had thought the matter out logically, he would have seen that according to his own statement, there is no single feature of the "original" reading that is not correctly preserved in *some* version; none of the individual corruptions can therefore go back to a corrupt archetype of *all* the versions. The "original" reading must, if his argumentation is otherwise correct, have been found in the intermediate archetype "t" (supposing that there ever was such a thing), as well as in the Urpañcatantra. If

[1] The Southern Pañcatantra has what Hertel considers the corruptions of both Tantr. and Pūrṇ. in this verse. The verse does not occur in Simpl. or in Pahl., which Hertel strangely thinks is a sign that it was corrupt in their original. But both Simpl. and Pahl., especially the latter, omit many verses of the original whose readings are open to no suspicion of corruption.

not, then one or the other of the later versions must have changed back secondarily to the original reading; and H. does not claim, apparently, that this was the case.

Hertel's first instance requires more careful consideration. In Tantr. " A 149 " (shortly after the verse II. 32) all Hertel's MSS. read *pratyarthito 'ham bhavatā*, " I have been challenged (or, opposed) by your worship ". Hertel emends *pratyarthito* to *pratyāyito*, " I have been convinced (persuaded, made confident) by your worship ". The Pahlavi must have read something like "requested" for the word in question, which, as H. says, seems to indicate a Sanskrit *prārthito* or the like, unless the Pahlavi translator took *pratyarthito* (wrongly) in the sense of *prārthito*. In the textus simplicior and in Somadeva the passage is wholly changed.

On the other hand, Pūrṇabhadra has *pratyāyito*. So have recensions α, β and γ of the Southern Pañcatantra (recension δ, evidently by corruption, *pratyāçvāsito*). In the Nepalese verse-collection the passage, being prose, is not found. The Hitopadeça has *āpyāyito*.

Now Hertel says: *pratyāyito* (which he puts by emendation in Tantr., and believes to have been the Urpañcatantra reading) is evidently the reading of the Southern Pañcatantra archetype. " Trotzdem wird sie der Quelle desselben—n–w— abzusprechen sein ". Why? Because the Hitopadeça has the reading *āpyāyito*, which "kommt in seinen Schriftzügen den anderen Lesarten so nahe, dass man wird annehmen müssen, es sei aus einer Korruptel [1] (!!) hervorgegangen, die Nārāyaṇa konjecturell besserte ". This hypothetical (or imaginary) " Korruptel ", Hertel tacitly assumes, must have been in " n–w ",[2] the original of both SP. and Hit., and consequently it must have been changed back again to the right reading in SP. Thus by piling sand on sand Hertel arrives at this con-

[1] Why may it not be itself a "Korruptel", or a deliberate or accidental change of some sort, from *pratyāyito*—which is the reading of SP. (Hit.'s closest relative), and which in its "Schriftzügen" comes closer to *āpyāyito* than the reading of the word found in any other version? Does Hertel mean to maintain that wherever Nārāyaṇa has departed from his original, that original must have been corrupt?

[2] Why, by the way, if the Hit. must be supposed to go back to a corruption at all, may not that corruption have crept in first in " n–w [2]", the intermediate stage between " n–w " and Hit.?

clusion: the corrupt reading *pratyarthito* was found in "t", from which all existing Pañc. versions are descended. It was taken over thence into the Tantrākhyāyika, and into the Sanskrit original of the Pahlavi (unless it read *prārthito* instead). This corruption was still found in "n–w", the common original of SP. and Hit. The SP. redactor "happily" corrected it to *pratyāyito* (which the Ur-ur-pañcatantra, back behind "t", once had) ; and the redactor of the Hit. made an attempt in the same direction, and got as close as *āpyāyito*. Finally, Pūrṇabhadra (whose tradition, according to Hertel, is in no way related to that of SP.) also hit upon the same very "happy correction" of his (necessarily corrupt, though unknown) original, and reads *pratyāyito*.

To me the following theory seems to explain the facts as stated much better. The original reading of the Urpañcatantra was *pratyāyito*; and if there was any such intermediate codex as "t", it likewise read *pratyāyito*. (I agree with Hertel that the sense of the passage requires this.) In the Tantrākhyāyika, or at least in the few MSS. of it we know, .this is corrupted to *pratyarthito*. In the original of the Pahlavi it probably became *prārthito* (whether independently of the Tantrākhyāyika corruption, or whether they both go back to a common original—a corrupt intermediate "t[1]" of their own, if you like—must be left in abeyance). In the original of SP. and Hit., on the other hand, as well as in the source used by Pūrṇabhadra at this point,[1] the original reading *pratyāyito* was preserved unchanged. The Hit. reading *āpyāyito* is surely much closer "in seinen Schriftzügen" to *pratyāyito* than to *pratyarthito*, and the inference to me seems obvious that it goes back to the former, and was introduced by Nārāyaṇa or by his hypothetical immediate predecessor "n–w[2]"—whether you call it a "konjecturelle Besserung" or simply a change, deliberate or accidental. Why chase all round Robin Hood's barn and assume an old corruption, only in order to assume again a ("happy" indeed!)

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[1] It seems to me quite conceivable that Pūrṇ. got the reading from a MS. of Tantrākhyāyika which antedated the corruption found in all our MSS. The number of MSS. of Tantr. now known is very small, and really smaller than it sounds, since Hertel has shown that some of them are only modern copies of one of the older ones. H. has also shown that Pūrṇ. made extensive use of a Tantr. codex.

correction of that corruption—a restoration of the original reading, and that too in *two independent versions* (SP. and Purn., both of which have the correct reading *pratyāyito*)?

II. A much more important statement of Hertel's "Stammbaum" is that all the older versions except the Tantr.—to wit, the Pahl., Bṛh., n–w, and Simpl., with Pūrn. except where the latter borrowed from Tantr.—go back to a common archetype, which Hertel calls "K", distinguishing the Tantr. archetype as "Ś". If true, this would obviously be of great importance in judging all variations between the several versions, and in reconstructing the Urpañc. Of course, the only way in which such a proposition could be proved is by showing common *changes* or *corruptions* in all these versions, as against the correct or original readings of Tantr. No amount of agreements in correct and original readings of these "K" versions, as opposed to corruptions in the Tantr., could have any bearing on the question. Furthermore, it must be shown in each case that *the same* change or corruption lies at the bottom of the text of all the "K" versions.

Hertel apparently recognizes this principle, and in fact undertakes to show in a few individual cases (he enumerates only four in his first statement of the case, Tantr. Uebers. I. 28 ff.) that all the "K" versions go back to a single corruption, while Tantr. in each case preserves the original. He thinks that he thereby proves absolutely the reality of his "K" archetype. I cannot agree with him, for two reasons. First, the cases he mentions are individually inconclusive. Secondly, they are too few in number to prove the point, even if they were individually sound.

In the first place, as to the few particular instances in which Hertel thinks the "K" versions go back to a common corruption, while the Tantr. preserves the original reading, I think that they all permit, and some of them demand, different interpretations. They can hardly be said even to *tend* to prove his point.

(a) One of them, to which Hertel pays a great deal of attention, occurs in the story of the Monkey and the Crocodile, the frame-story of Book IV. (Of this story, in all important versions, H. gives an interesting and valuable com-

parative translation, in parallel columns, Tantr. Uebers. I.
70 ff.)   Those who do not know the story (which is a very
wide-spread one) may easily locate it in any Pañcatantra
translation.   The point which concerns us here is that in all
the versions except the Tantr. the treacherous crocodile is
represented as inviting his unsuspecting friend, the monkey, to
get on his back and visit *his own house*, meaning by this trick
to compass his death.

Now, says Hertel (l. c., p. 89), in the Tantr. the crocodile
does not make this "absurd proposal" that the monkey should
visit his own house—"das liegt ja im Wasser";[1] instead, he
proposes to take the monkey, on his own back, over to a lovely
island, where the monkey may enjoy all sorts of sensual de-
lights (Tantr. A 286).

Just in this difference lies the point which Hertel finds so
important.   For later, when the monkey discovers the croco-
dile's trick, he is represented (in all versions) as bewailing his
own *love of pleasure*, which had enticed him ("in spite of his
age") into agreeing to the suggestion of his false friend.
.Hertel thinks this is inconsistent with the account in the other
texts: "in allen (anderen Versionen überlistet) der Śiśumāra
(crocodile) den Affen nicht durch die Aussicht auf sinnliche
Vergnügungen mit jungen Äffinnen, sondern durch eine Ein-
ladung in sein Haus".   The "junge Äffinnen", it should be
said, appear even in Tantr. only through an emendation of
Hertel's; the emendation is, however, a very attractive one,
and probably the original contained something of the sort.

But it seems to me that Hertel attaches insufficient impor-
tance to the references which even the other ("K") versions
contain to sensual delights as a motive for the trip to the
island.   (Cf. the Pahl. and Simpl. readings parallel with
Tantr. A 286, Hertel, l. c., p. 84, 85.)   In the Pahl. (and
seemingly in Simpl., though its account is compressed) the
invitation to visit the crocodile's home is *coupled with* a
promise of various sensual (not, indeed, sexual) joys.[2]   So

[1] But in Simpl. and Pahl. the crocodile distinctly tells the monkey
that his house is *on the island*. This may have been a lie, but the
monkey could hardly be expected to know it. So the proposal is not
at all "absurd".

[2] SP., Som. and Kṣem. have lost this entire paragraph in their very
abbreviated versions.

that Hertel's sentence quoted above, while literally true, is un-intentionally misleading; the other versions *do* motivate the seduction of the monkey *in part* "durch die Aussicht auf sinnliche Vergnügungen". That the original made it spe-cifically *sexual* pleasures I am inclined to believe with Hertel; this feature of the original is indicated by the later *dénoue-ment* in several versions (including SP.), although at the point we are now considering it does not come out clearly in any (not even in Tantr., unless by the grace of Hertel's emendation).

But this is a matter of minor importance. The fact re-mains that in all the versions but Tantr. the crocodile invites the monkey *to his own house*, promising as an added induce-ment various sensual delights on the island where he says his home is. In the Tantr. (A 286) where the crocodile first speaks of the island with its sensual delights, there is indeed nothing said about the crocodile's *home* being there. And in all the conversation between the two, as reported in the Tantr., the crocodile never mentions his house. He merely expresses his sorrow [1] at never having made any return for the favors shown him by the monkey (A 281 f., Hertel, l. c., 81–83).

But note the monkey's strange reply to this expression of regret! I quote the monkey's words (Tantr. A 284, Hertel, l. c. 83) : "And as for your saying, 'I have never invited you *to come to my house*, to meet my wife, and to eat from my dish', what difference does that make? Such is the friend-ship only of common people ! "

Now according to the Tantr.'s text as it stands, the croco-dile had not said anything of the sort at all! Just therein, according to Hertel, lies the great superiority of the Tantr.'s text over the other—"corrupt"—versions, and the striking proof that all the others go back to a single corrupt arche-type "K"!

It seems evident that in his haste and enthusiasm Hertel quite overlooked these significant words of the monkey's. They seem to me to show that the archetype of the Tantr. must have agreed essentially with the other versions. The monkey is represented as quoting the crocodile's very words, which consist precisely in that invitation to his hearth

[1] A frivolous query: Is this the origin of "crocodile tears"?

and home which Hertel thinks cannot have been in the original.

Instead of proving that the other recensions go back to a corrupt archetype, this example proves that the Tantr. is corrupt at this point. Its version is glaringly inconsistent with itself. Somehow or other it has lost from its text the preceding words of the crocodile, containing the suggestion that the monkey come to his house, which alone make the words of the monkey in A 284 intelligible.

The Jātaka versions of the fable (Jāt. 208, 342) say nothing about any invitation to the crocodile's house, and of this Hertel makes much, claiming it as a confirmation of what he takes to be the original form of the story. But in the Jātakas the story is otherwise changed in such a way that it would have been rank absurdity to keep the incident of the invitation. The monkey and the crocodile are not friends, nor even acquaintances, in the Jāt. story, and it would have been absurd—not to say suspicious—if the crocodile had invited the monkey to his house. The Jātakas are characteristically intent on the moral lesson—the dangers of sensuality—and leave out all the rest of the story.

(b) Hertel refers (Tantr. Uebers. I. 31 f.) to the introduction to his SP. (p. LXIII ff.) for another instance which he thinks points to a corrupt archetype ("K") from which all versions except the Tantr. are descended.

This concerns the strophe SP. II. 41 = ν I. 47 = Hit. I. 129 Pet. = Tantr. II. 90 = Pūrṇ. II. 116. This stanza contains two words in which Tantr. differs from the consensus of Pūrṇ. and the descendants of n–w (that is, SP., ν and Hit.). Hertel thinks that the Tantr. reading for both these words is clearly the original, and consequently that the other versions all point to a corrupt archetype.

In the first place, this strophe is not found in Pahl., Som., Kṣem. or Simpl., so that it is surely somewhat rash to say that it proves a corruption in an archetype to which all these versions go back. The most that it could prove would be a corruption in some common archetype of n–w and Pūrṇ., where alone the verse is found (outside of Tantr.). But it does not prove even that.

I will quote the Sanskrit text indicated for the common archetype of n–w and Pūrṇ. and assumed by Hertel as the reading of his postulated " K ":

na svalpam apy adhyavasāyabhīroḥ karoti vijñānavidhir
　　　guṇaṁ hi
andhasya kiṁ hastatalasthito 'pi nivartayaty artham iha
　　　pradīpaḥ.

There are minor variations which do not concern us; thus Pūrṇ. reads *nivartayed* in d for *nivartayaty*, Hit. and *v* read *prakāçayaty*, and most MSS. of SP. *sandarçayaty*, for the same word; but the best MS. of SP. points to an archetype agreeing with Tantr. as to this word.

Now the Tantr. text agrees almost wholly with the above, but reads *avyavasāyabhīroḥ* in a, and *āndhyam* for *artham* in d. These readings Hertel thinks are original, and he infers that n–w and Pūrṇ. go back to a common corrupt archetype. Decidedly a *nonsequitur*, for H. himself says (Pañc., p. 76) that Pūrṇ. used "several Pañc. versions which are unknown to us " (I should add, "and probably other sources, including oral *geflügelte Wörter*, not attached to any literary work, many of which he incorporated among his verses "). It is, then, quite conceivable that Pūrṇ.'s reading of this stanza may have been taken as a whole from Tantr., or from some source having the Tantr. reading, but Pūrṇ. may have changed these two words into accord with another version of the stanza which he knew from another source. So Pūrṇ. affords very slight evidence as to the supposed archetype " K ", and there is left only n–w; the " corruption " then, if it be a corruption, may have come in with n–w.

But is the reading of n–w and Pūrṇ. a corruption? It means: "The acquisition of knowledge does not confer the least advantage (*guṇa*—superior quality, superiority) upon a man who is afraid to take a firm stand (*adhyavasāya*, rendered *utsāha* in some Hindu lexicons, see BR.). If a lamp is placed right in the hands of a blind man, what advantage (*artha*) does it bring to him here?" Every Sanskrit word is here rendered in accordance with well-recognized meanings, and it seems to me to make excellent sense.

The Tantr. version means, according to Hertel: " The acquisition of knowledge does not give the least advantage (*Vorzug*) to a man who is irresolute and timid. Is the blindness of a blind man removed here by a torch, which he holds in his hand?" H.'s translation also makes good sense, though I cannot see that it is any better than the sense yielded by the other reading. The verb *nivartayaty* has to be taken in the sense of " remove ", instead of " confer "; strange to say, both are well-attested meanings of the word. H. claims —rightly, it seems—that " remove " is the commoner meaning. If so, it would suggest to my mind not that it was the original meaning in this place, but rather if anything the reverse (the *lectio difficilior* is, other things being equal, more apt to be original). And the reading *āndhyam* for *artham*—with the word *andhasya* in the preceding pada—sounds like a secondary rather than a primary reading. Is not the following at least as likely a theory as Hertel's? The Urpañcatantra read in pada d *nivartayaty artham*, " confers advantage ". The Tantr. redactor was more familiar with *ni-vartayati* in the sense of " remove "; the word *artham* as written in the Çāradā alphabet is (as H. observes) easily confused with **antham*, which—under the psychological suggestion of the word *andha* in the preceding pada—naturally suggested *āndhyam*, " blindness ", as the thing which the lamp would not " remove ". So arose Tantr.'s reading. I do not say, Hertelwise, that this is the only conceivable explanation of the variation in pada d; I do submit that it is at least as likely as H.'s contrary explanation (personally it seems to me much more likely), and therefore that " völlige Sicherheit " can hardly be claimed for Hertel's opinion.

Similarly as to the other variation in this verse, that of pada a, in which Tantr. has *avyavasāyabhīroḥ* for *adhyava°* of the others. Hertel must of course take the Tantr. reading as an adjectival dvandva compound, " irresolute and fearful ". He thinks this makes much better sense than "afraid to take a firm stand " or " to make a firm resolve ". *Geschmacksache*; if Hertel likes it better, we must allow him his preference. To me the other meaning seems quite as sensible, and it accords better, if anything, with Sanskrit usage. I am suspicious of " adjectival dvandvas " both members of which are

supposed to apply to the same individual.[1] Such forms are in any case not common except with color-adjectives. But let that pass; it too may be called "subjective". What I want to emphasize is that Hertel's purely subjective liking for one variant as against the other can hardly be considered proof that the one was original. Others, of whom I am one, happen to like the other better. Hertel does not deny that the other makes sense. His attempt to make it seem poor sense seems to me unsuccessful.

Hertel points out that the variant is a graphic one; *vya* and *dhya* are closely similar in the Çāradā alphabet. Since he assumes *vya* as the original, he infers that the *dhya* of n–w and Pūrṇ. must go back to an archetype written in Çāradā, though (I believe) none of their existent MSS. are written in that alphabet. This is a theory; it is a *fact* that all our MSS. of the Tantr. are written in Çāradā. It seems that it would be more *naheliegend* to assume that our Tantr. MSS. have corrupted an original *dhya*, written in Çāradā characters, to *vya*.

On this point again I do not claim that the explanation I offer is the only conceivable one. I merely maintain that it is quite as likely to be true as Hertel's, and therefore that Hertel is deceived in thinking that his is the only conceivable one.

My argument on this verse contains three points. (1) The verse is not found in Pahl., Som., Kṣem., or Simpl., all of which H. assumes to be derived from "K", and therefore— being based only on n–w and Pūrṇ.—the verse offers no reliable evidence for "K". (2) It does not even offer reliable evidence for a hypothetical archetype of n–w and Pūrṇ. alone, since it is easily conceivable that Pūrṇ. got his readings for the verse from a source dependent on n–w or from a source outside the Pañc. circle. (The verse is of the sort which might be used anywhere at all.) (3) Most important of all: the reading of n–w and Pūrṇ. is more likely to be the Urpañc. reading than that of Tantr.

[1] The Hindus—rightly, it seems to me—treat such forms as karmadhārayas (e. g. "irresolutely fearful") ; Delbrück, AIS., p. 73, takes the same view, but the high authority of Wackernagel is on the other side (A–I. Gram. II. 1, Section 74 a).

If I am right in any one of these three contentions, then another of Hertel's "proofs" for "K" has been shown to be a mere subjective impression, and an improbable one.

(c) Another passage—and one which Hertel considers his most powerful argument for the "corrupt archetype K"—is discussed in his Pañc., p. 440 ff. (cf. further Tantr. Uebers. I. 28 ff., 158). It concerns a supposed lacuna which he thinks he can establish in "K" in the story corresponding to Tantr. II. 2. The *Kathāsaṁgraha* or catch-verse to this fable reads in Tantr. thus:

> *nākasmāc Chāṇḍilī mātā vikrīṇāti tilāis tilān*
> *luñcitāṅl luñcitāir eva kāryam atra bhaviṣyati.*

That is: "Not without reason does Mother Çāṇḍilī offer to sell sesame for sesame, huskt for likewise huskt; there is surely something in that ('a nigger in the woodpile')".

In the prose text of the story, according to Tantr., Çāṇḍilī sends a boy to exchange some huskt white sesame, which had become defiled, for black sesame, insisting that it should be likewise huskt. She thus hopes, by emphasizing the equality of the bargain, to escape the natural suspicion that there was something wrong with her sesame.

In most of the other recensions the catch-verse is changed in such a way as to eliminate the demand for huskt sesame in exchange for huskt. Only in one MS. of Simpl., and in certain descendants of Simpl., do the words *luñcitāṅl luñcitāir* (or equivalent), "huskt for huskt", occur in the verse. I think Hertel is probably right in maintaining that this is the original reading of the verse; the other versions are then corrupted (or at least changed) at this point. Nevertheless they cannot go back to a corrupt archetype. "K", if there ever was a K, must have had the original reading (as in Tantr.) at this point; this is shown by Simpl., and is admitted by Hertel. All the offshoots of K, then, except some texts of Simpl., have become *independently* corrupted in this verse, according to Hertel himself.

But, says Hertel, all the "K" versions (except Bṛh., cf. below) in the prose text of the story say that Çāṇḍilī tried to exchange her huskt sesame for *unhuskt*. This is true even of the one MS. of Simpl. which in its catch-verse speaks of

"huskt for huskt". In this MS., then, the prose is inconsistent with the verse; and in all the "K" versions except Bṛh. the prose is inconsistent with the assumed original form of the verse (though not with the verse as read in these versions themselves).

So far I can find no fault with Hertel's reasoning. I agree with him that the evidence seems to indicate a departure from the original point of the story in most of the versions. And the departure is in all of them the same as to general sense, though not as to verbiage; this would scarcely be expected in the prose. I should further admit that if a *very large number* of cases could be found in which the so-called "K" versions similarly agree in readings which can be shown to be secondary or corrupt, as against the Tantr., then there would be reason for assuming Hertel's "K" archetype. But since this is the only case in which it seems to me that Hertel has made such a thing appear even plausible, and since equally or even more plausible cases could be made out for "corrupt archetypes" of any two Pañc. versions you could name (see below, p. 275 ff.), I do not admit that this single instance proves anything; nor would any two or three, or even ten or a dozen similar instances prove anything. For anything like proof we must expect cases which are either much more numerous, or much more compelling—preferably both. On the contrary, in view of all our present knowledge it seems to me much more likely that the change—or corruption, if you like—came in independently in all the "descendants of K". In any case, I think that there is no reason of weight for assuming that precisely a "lacuna" is indicated in "K" at this point, as Hertel does. On the contrary, as I shall show in a moment, I think that the occurrence of an identical proper name in both Tantr. and SP. at the place of the supposed lacuna shows that there was none.

That the same corruption could occur independently in many versions is admitted by Hertel, both for the catch-verse of this very fable, and in other connexions; thus in his Tantr. Uebers. II. 17, note 1, he assumes for the original Pañc. a passage which is found *only in Tantr. and Hit.* Here then is a "lacuna" which has crept in independently in all of the "K" versions which have the assumed corruption in the

sesame story.   Why then should H. assume that in the sesame story this precisely similar corruption (for so it is, according to his theory—a lacuna) proves a corrupt archetype ?

His arguments for this assumption seem to me both subjective and flimsy, and beside the point.   Thus, he says the story as told in Tantr. is much more clever; no one, he says, could possibly have been deceived by an offer to exchange huskt for unhuskt sesame.   Even if this were admitted, it would have no bearing on the question.   Whether stupid or clever, the alteration in the story must have been made at least once; why may it not have been made three times?   Moreover I do not find the "K" form of the story so inconceivably stupid. A closely analogous motive occurs in the familiar story of Aladdin in the Arabian Nights ("Tausend und Eine Nacht") ; the offer of "new lamps for old" worked successfully there. Evidently to the oriental mind, at least, this idea was not too stupid to pass in a story.   It seems to me that there are in the Tantr. itself much more stupid and banal stories than the "K" version of this story.   The latter seems to me, in fact, a more natural turn for the story to take than the almost *too* clever version of the Tantr.   I can very easily understand the repeated change, in several (three) versions independently, from the very *recherché* to the more simple trick.   The change, furthermore, may easily have been a mere verbal slip; it may have arisen simply from a mistaken assumption of an *a*-privative in *sandhi* (*luñcita* : *aluñcita*).   We do not know what the original text of the Pañc. was at this point.

It should further be emphasized that both Som. and Kṣem. (descendants of "K", according to Hertel) say nothing about either huskt or unhuskt sesame.   Their accounts of the story are, as usual, abbreviated.   Som.'s version, in fact, is so brief that it does not bring out clearly the point of the story. Hertel's theory—more ingenious than probable—is that Som. still shows in his text the "lacuna" which he assumes for "K" at this point, and which he thinks has been "falsely restored" in the other versions.   He does not say how he interprets Kṣem.'s version, which summarizes, though briefly, the part of the story omitted in Som., and which goes back to the same immediate original with Som.

But Som. and Kṣem. are frequently so abbreviated that

even important points of the story are obscured or omitted; this by no means indicates a lacuna in their original. SP. contains, in the part of the story omitted in Som. and therefore, apparently, in the supposed "lacuna" of "K", the proper name Kāmandaki, which is found at this point in Tantr. and could scarcely have been supplied from the imagination of the SP. redactor. This militates against the assumption of a lacuna between SP. and the common archetype of it and Tantr. And in general the versions of SP., Pahl. and Simpl. agree quite as closely as usual with the version of Tantr. throughout this story. The only variation of any consequence is the change of huskt to unhuskt; this, as I have indicated, may rest on a very slight verbal misunderstanding, and in any case much more serious variations than this occur repeatedly in all of these versions individually. That in this particular case the same corruption should have occurred in all three is indeed worth noting, but need not prove anything except that the corruption was a very easy and natural one. Even Hertel admits, as I have said, similar independent but identical changes for these versions at other points.

Recapitulating, my position is that Hertel's theory of the passage, while abstractly not impossible, is certainly not definitely proved—to put it very mildly. Another theory seems at least as liable to be correct (much more liable, in my opinion), namely, that the change of the word "huskt" to "unhuskt" in the prose story occurred independently in the texts (or the archetypes) of Pahl., n–w and Simpl.—exactly as, according to Hertel's own admission, this same change did occur independently in the catch-verse of the story.

(d) The only other case for the "archetype K" mentioned in Hertel Tantr. Uebers. I. 28 ff. is the verse Tantr. II. 25 (l. c., p. 32, note 1), in which he claims that all "K" versions read *sutaptam* wrongly for *ataptam* of Tantr. (A more lengthy discussion of this WZKM. 25. 13 ff.) The Tantr. stanza means: "With an enemy one should not ally himself, not even by a very intimate alliance. Water, even though not heated (or, punningly, "not injured"), puts out fire". The other versions, reading *su-* for *a-*, mean: "... Water, even if it be heated hot (i. e. even if its nature be changed so as to be as close to the nature of fire as possible), still puts out fire".

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This seems to me to make as good sense, and to be as suitable to the context, as the Tantr. reading. Even if it is a secondary reading (which I am far from admitting), it depends on a very slight verbal change, which is furthermore suggested by the most natural meaning of the root *tap* in such a connexion. Water is very frequently " heated "; it is hard to conceive of its being " injured ". It is only through the *double entente* in the word *a-taptam* that the Tantr. has any meaning at all. Therefore it would be easy to suppose that the corruption—if it be one—occurred independently in several versions, which failed to see the rather *recherché* meaning of the reading with *ataptam*, and thought they saw a chance to make the verse sensible. And in fact, the " K " reading is quite as good, and quite as apt to be the original Pañc. reading, as the other; in my opinion, more so.

(e) In WZKM. 25. 9 ff. Hertel adds another passage which he thinks points to a corrupt archetype " K ". This is Tantr. I. 19, in which Tantr. reads *dhūrtaṁ tam*, while the " K " versions have *dhunvantam*. The two readings are easily confused in the Çāradā alphabet, and the most natural supposition would be that the variant or corruption came in in the one version of the Pañc. whose MSS. are written in that alphabet —namely, Tantr. Of course, Hertel assumes the opposite.— He translates *dhūrtaṁ* "verschlagen", "cunning", which must be taken (as the context shows) in a good sense, "clever ". But *dhūrta* seems always to be used pejoratively; it means "a sly rascal, a cheat ". This does not at all fit the verse; the reading *dhunvantam* does fit it.—Hertel (l. c., p. 11) refers to the reading *dhūnvantam* of some SP. MSS., and says that the " false " *ū* points to the *ū* of *dhūrtaṁ*. The *ū* is not at all false; the root *dhū* is a well-recognized by-form of *dhŭ*, and the present *dhūnoti* is found both in the Veda and in Classical Sanskrit; see BR., and Whitney's " Roots, Verb-Forms ", etc., s. v.[1]

[1] On p. 18 of WZKM. 25 I note what seems to me an astonishing remark from a scholar so familiar with the habits of Indian MSS. as Hertel is. "The reading of the majority of the MSS., FOEI, is perfectly comprehensible; therefore the reading of HM cannot be a correction of that of FOEI (!). Graphically, also, the reading of HM cannot be explained from that of FOEI. So there must have been

III. So much for Hertel's arguments about his archetype "K". On equally weak, or even weaker, grounds rests his assumption that Pahl., n–w (SP., *v*, Hit.) and Simpl. (with Pūrṇ.) go back to a nearer archetype, which he calls "N–W", a descendant of "K" of which Som. and Kṣem. are independent. So far as I can see, his reasons for this are just two. One is that in the sesame-story above mentioned (p. 266 ff.) those versions seem to him to have "restored" in a similar way the "lacuna" of "K", which Hertel thinks is still found in Som. This restoration, he says, must go back to an archetype of these versions. As I have already indicated, the lacuna and its restoration seem to me alike imaginary.

The only other attempt to prove the reality of "N–W" which I am able to find in Hertel's writings is that treated in Tantr. Uebers. I. 32 f. (cf. also SP. Einl. XXXVI ff.), the story of the brahman and the rogues (Tantr. III. 5). It is necessary here again to distinguish between what Hertel has a sound philological basis for asserting, and what he merely conjectures on purely subjective grounds; unfortunately he states both things with equal assurance. In the text of SP. (or at least in an archetype of all of Hertel's MSS. thereof except possibly T and X, both secondary MSS.) there was a lacuna in this story. I do not see how anyone could doubt this, after inspecting the table of the MS. readings on SP. Einl. XXXIX, and comparing the text, p. 47, with the critical notes thereto, p. 104. The point is almost superficially obvious; in a large group of MSS. the gap is still there. But in extending this lacuna back to a hypothetical common archetype of SP., Simpl. and Pahl. it seems to me that Hertel again

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some special reason for the change . . . ". A most amazing proposition, that there must be some "special reason" for every little variation introduced by a stupid, slovenly Hindu copyist. The change in question, for which Hertel thinks a "special reason" must be sought, is a shift between the syllables *ti* and *tya*. I am sure that Hertel knows as well as I do that much more serious changes occur constantly in Hindu MSS. without any "special reason" except the carelessness and ignorance of copyists. I only mention this remark—which I am sure Hertel would not defend in cold blood—to show how he is liable to be entirely carried away by the desire to prove a point, so that he momentarily fails to see the most commonplace things in their true light.

becomes subjective and wholly inconclusive. That Pahl. and the Jaina recensions speak of only three rogues,[1] while Tantr. and Som. speak of six (who however appear in three groups —first one, then two, then three), is surely no matter for surprise, and scarcely even calls for comment. In Tantr. and Som. first one rogue speaks to the brahman, then two, then three. The brahman is only addressed three times; why (say the later redactors) should there be more than three speakers? The climactic arrangement of the numbers is exactly the sort of trifling detail which we find later redactors constantly altering, either carelessly, or deliberately (because there seemed to be no reason for it). The only reason, indeed, which Hertel can think of for its being used in the original is that perhaps the author wanted to give examples of parallel Sanskrit forms in the singular, dual and plural. The passages as given in Pahl. and Simpl. are just as well and as fully told as in Tantr. Nor do they differ from Tantr. any more than usual, if as much. It seems scarcely believable that the trifling change about the numbers should seem to Hertel to indicate a "lacuna" in the archetype of these versions; yet such appears to be his argument. If there were marked variations—especially absurd or inconsistent ones—in the story, as there are in the individual MSS. of SP., whose text actually has a plain gap in many MSS., then indeed something of the sort might be assumed with more speciousness; although I have indicated above the danger of basing arguments on even such grounds.

I have tried to show that the arguments advanced by Hertel in support of his "K" and "N-W" are individually inconclusive. It seems to me that they are open to this more general criticism: the number of instances he adduces is too small to prove anything anyhow. Even if it were true that in these few

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[1] In Old Syriac there are four; but the Arabic and its descendants make it clear that Pahl. had three. The slight variations in the number which appear in descendants of Pahl. show how absurd it is for Hertel to make so much of this infinitesimal point. Some of the descendants of the Arabic even have only two rogues. Yet we *know* that Syr., as well as Arab. and all its descendants, came from one single text— Pahl.—not from several "archetypes"—and that *that* text had some perfectly definite number here—whether four, three or two. Yet its descendants vary.

instances the Tantr. alone has the original reading (and there is only one instance in which there is good reason to assume this) and even if the other versions did presuppose an identical corruption, it is quite possible to believe (as I have already hinted) that these few changes crept in independently in the different " K " or " N–W " versions.   My experience with the same problem—which confronted me in dealing with the inter-relation of the versions of the Vikramacarita—leads me to the conviction that the problem is far from being so simple as H. seems to conceive it.   Hindu literary tradition is such a terri-bly complicated matter, that in no work of the size of the Pañc. or the Vikramacarita could interrelationship be decided by any three or four, or half a dozen, cases of agreement or disagree-ment in detail—however striking.   By such arguments the close connexion of any two different recensions of any Hindu work could be proven.   When my " composite outline " of the four principal Vikr. recensions is published,[1] it will show at a glance the truth of my statement.   This " composite outline " summarizes the entire work in all versions, and shows that each version agrees with every other version in a certain num-ber of corruptions or changes, as well as in original readings. I can also show that the same holds good of the Pañc. versions. A few examples will be given in a moment.

But genetic relationship must rest on much broader consid-erations than this: on sweeping and extensive changes from the original plan of the work as a whole, or on extensive and far-reaching verbal agreements, or on a *very large* number of common corruptions or changes in detail.   On such broad and sound considerations Hertel bases his statement, for instance, that SP., *v* and Hit. are closely related, and that the latter two are closer to each other than to SP.; also, that Pūrṇ. used both Simpl. and a Tantr. codex for his new version.   But up to this time Hertel has produced no proof of *this* sort for his archetypes " K " or " N–W ".   Until he does so he ought not,

[1] My two-volume work containing a complete text and translation of the main versions of Vikr. in horizontally parallel sections, with intro-duction, critical apparatus etc., has been in the hands of the printer for nearly two years; the delay has been in no way my fault, and has been much more deeply deplored by me than it could have been by anyone else.

it seems to me, to claim that these hypotheses represent any-
thing more than his personal opinion and impression. As
such they are certainly entitled to respectful consideration,
although after a careful study of them I have been forced to
disagree. But certainly they cannot properly be used as
established facts in demolishing his critics (as Hertel does
use them repeatedly; see for instance ZDMG. 68. 66).

Now in order to show that such cases as Hertel uses to
prove his archetypes " K " and " N–W " may be found to es-
tablish archetypes for any two recensions—or even sub-recen-
sions—of the Pañc., and thus to furnish a *reductio ad absur-
dum* of his argument, let me tabulate a few striking cases of
variant readings, all taken from the verses of Book I of the
Pañc., which I have carefully studied from Hertel's very use-
ful tables of correspondences.[1] It will appear from these
materials that, by picking out here and there a few verbal
correspondences or common variations, such as Hertel uses to
establish his "K", it would be possible to prove that Tantr. *α*
and SP. *α* go back to a common archetype different from
Tantr. *β* and SP. *β*, or that Tantr. and *ν* are similarly con-

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[1] Tantr. Uebers. I. 100 ff., Pañc. 78 ff. I have discovered a number of
errors (some of them, I presume, misprints) in these tables. In the first-
mentioned correct as follows: *p. 100*, opposite Śār. I. 2, insert in Pa.
column "Syr. Schulthess p. 1, l. 21 ff."; *p. 104*, opposite Śār. (Hertel's
abbreviation for Tantr.) 65, read Pa. 57 for 55; opposite Śār. 128,
insert Kṣem. 82 cd, 83 ab; below SP. 63 insert 64 (= Pūrṇ. 230) with
dashes in all the other columns; Śār. 73 corresponds to SP. 66, *ν* 49,
Pa. 51, and Som. 119 as well as to SP. 65 etc. (cf. my remarks below,
p. 278); opposite Śār. 78 insert Pa. A 37 (?); *p. 105*, opposite Śār. 92,
insert Pa. A 55, end; *p. 106*, opposite SP. 106, read Pa. 76 a instead of
A 65; *p. 107*, opposite Śār. 126, read Pa. 81 instead of 79; *p. 109*, oppo-
site Śār. 168, in Kṣem. column delete *ab*.—In the second-mentioned
table (Pañc. 78 ff.) correct as follows: *p. 78*, read Śār. Kathāmukha 3
instead of 1 (opposite Pūrṇ. Simpl. 1); read Simpl. Kathāmukha 6 for
7; opposite Śār. I. 1 insert Simpl. 1; opposite Śār. 22 insert Simpl. 63;
p. 79, the Simpl. references opposite Pūrṇ. 219 really correspond to
Pūrṇ. 220 (which is omitted by mistake) and Simpl. II. 157 should be
read for II. 160; Śār. 73 = Pūrṇ. 232 ab and 233 cd (cf. above, and p.
278 below); footnote 8, read 280 for 278; *p. 80*, below Pūrṇ. (Story)
XIV, insert as follows: Śār. 120 = Pūrṇ. 310, Śār. 122 = Pūrṇ. 311 =
Simpl. 308, Śār. 124 = Pūrṇ. 312 = Simpl. 309; also, below Śār. (Story)
XIV insert as follows: Śār. 161 = Pūrṇ. 384 = Simpl. 394, Śār. 162-3 =
Pūrṇ. 385-6.—Furthermore: Pūrṇ. 404 = 409, Simpl. 409 = 414.

nected, etc. Since of course we know that these conclusions would be unwarranted, I think it is thereby proved that Hertel's method of proving his archetypes "K" and "N–W" is unsafe and unscientific.

(a) Tantr. I. 5 = SP. I. 7 = ν II. 4 = Pa. I. 2 = Pūrṇ. I. 8 = Simpl. I. 21. (Correspondents in Bṛh. furnish no evidence on our point; neither does Pa.) In pada c, Tantr. text (with a) reads *sa naro nihataç çete*; SP. text, and ν, *sa bhūmāu nihataḥ çete*. The reading of Simpl. and Pūrṇ., *sa eva nidhanaṁ yāti*, would seem at first sight like a secondary change. But looking at the variants of Tantr. and SP., we find that Tantr. β reads *sa narah pralayaṁ yāti*; and the MS. of SP. which Hertel considers on the whole the most original, viz. K, reads *sa eva nilayaṁ yāti*, which looks like a cross between Tantr. β and Simpl.-Pūrṇ., while three MSS. of SP. β read exactly as Simpl.-Pūrṇ. Now try to construct archetypes, Hertel-fashion, for these readings, and what do we get? On the one hand—

Tantr. a:	*sa naro nihataç çete*
SP. (most MSS.) and ν:	*sa bhūmāu nihataḥ çete*

On the other hand—

SP. β 3 MSS., Simpl., Pūrṇ.:	*sa eva nidhanaṁ yāti*
SP. a 1 old MS.:	*sa eva nilayaṁ yāti*
Tantr. β:	*sa narah pralayaṁ yāti*

Apparently, therefore, Tantr. a, ν and most SP. MSS. go back to one "archetype", while Simpl., Pūrṇ., one SP. a MS. and 3 SP. β MSS. go back to another, which is also the principal basis of Tantr. β, though that has taken the word *narah* from the other!

In particular, the ending *nihataḥ çete* is found in good descendants of both Hertel's "K" and his "Ś"; likewise, the ending *yāti* (for *çete*) preceded by an accusative. What becomes of the "archetypes K and Ś"? How did each of them read here? "Fortunate restoration" will hardly serve to explain such a change as this. It must be due to the floating and indefinite character of the Hindu literary tradition, which refuses to be run into theoretical molds to the extent that Hertel tries to do so.

(b) Tantr. I. 10 = SP. I. 13 = *v* II. 9 = Simpl. I. 24.
Pada d: Tantr. *a* and most SP. MSS. read *ciráya*; Tantr. *β*,
Simpl., *v* and two MSS. of SP. *a* read *ciraṁ ca* (one SP. *a*
MS. *cireṇa*). What did "K" read?

(c) Tantr. I. 59 = SP. I. 50 = *v* II. 36 = Pūrṇ. I. 159 =
Simpl. I. 207 = Kṣem. I. 28 cd, 29 ab. Padas cd: Tantr. *a*
and *β*, and SP. in all MSS. recorded by Hertel read *kākī* (SP.
β kākaḥ) *kanakasūtreṇa kṛṣṇasarpam amārayat*. But *v*
(the closest relative of SP.) reads, with Pūrṇ. and Simpl., in
d *kṛṣṇasarpo nipātitaḥ* (in c Pūrṇ. and Simpl. read *kākyā*,
v kākīkanaka°). The reading of *v*, Simpl. and Pūrṇ. seems
to be confirmed by Kṣem., which according to Hertel used
only Tantr. *β* besides its main source Brh. (a descendant of
"K"); Kṣem. I. 29 ab reads *çrūyate kṛṣṇasarpo hi dhiyā*
kākyā nipātitaḥ. What becomes of Hertel's archetype "K"
at this point? Did it agree with *v*, Simpl., Pūrṇ. and Kṣem.,
or with SP., the antiquity of whose reading is vouched for by
Tantr.? Can Hertel possibly maintain that SP.'s reading is a
"happy restoration"?!

(d) Tantr. I. 60 = SP. I. 51 = *v* II. 37 = Pūrṇ. I. 165 =
Simpl. I. 210. Pada c: all MSS. of SP. read *kaçcin*; so
likewise Simpl. and Pūrṇ., apparently establishing the reading
of "K". But *v* reads *paçcān*, with Tantr. Apparently, then,
v would belong to "S", the archetype of Tantr. Or is this
again a "fortunate restoration"?

(e) Tantr. I. 64 = SP. I. 56 = *v* II. 41 = Pūrṇ. I. 221. In
pada a: Tantr. *a* and SP. *a* read *abhyucchrite*, Tantr. *β*, SP. *β*
and Pūrṇ. *atyucchrite*. It would appear, then, that Tantr. *a*
and SP. *a* form one archetype, as against Tantr. *β* and SP. *β*.

(f) Tantr. I. 81 = SP. I. 73 = *v* II. 54 = Pūrṇ. I. 251.
Pada a: Tantr. *a* has *kuçalān*, *β kuçalo* (Hertel's text *'kuça-*
lān); SP. Pūrṇ. *vyasanān* (so establishing "K"'s reading),
but *v kuçalā* (with Tantr.!).

(g) Tantr. I. 105 = SP. I. 92 = *v* II. 71 = Pūrṇ. I. 245.
Pada b: SP. *β*, *v* and Pūrṇ. read *kamalaropaṇam*; SP. *a* with
Tantr. *'bjam avarop°*. What did "K" read?—The last parts
of padas b and c, as read in Tantr. and SP., are transposed in
v and in Pūrṇ. What was their order in "K"?

(h) Tantr. I. 153 = SP. I. 132 = *v* II. 106 = Pūrṇ. I. 373.
Pada a: Tantr. *a*, SP. *a* and *v* read *kaluṣeṇa*; Tantr. *β*, SP. *β*

and Pūrṇ. *kapaṭena.* (Hertel follows Tantr. *a* in his text of Tantr., but SP. *β* in his text of SP.)

(i) Tantr. I. 155 = SP. I. 133 = *v* II. 107 = Pūrṇ. I. 375. Pada c: Tantr. *a* with *v çaçāṅkasya*; Tantr. *β* with SP. and Pūrṇ. *saçāṅkasya.* Hertel thinks the reading of Tantr. *a* is the original, and that of *v* a "fortunate correction". The meaning of the verse is dubious; I am not satisfied with Hertel's interpretation.

(k) Tantr. I. 158 = SP. I. 136 = *v* II. 109 = Pūrṇ. I. 381. Pada b: SP. and Pūrṇ. have *tam anuv°*, Tantr. and *v samanuv°.*—Pada d: Tantr. Pūrṇ. *bhavaty āmalakīphalam.* The reading which would be indicated for "n-w", by the general consensus of the SP. *a* MSS. and *v*, is *jāyate* (or with *v jāyetāṁ*) *kaṇṭakaṁ phalam.* Nevertheless SP. *β* (a secondary recension) reads exactly as Tantr. Pūrṇ., except *āmalakaṁ* for *°kī.* Apparently SP. *β*, Tantr. and Pūrṇ. go back to a different "archetype" from SP. *a* and *v*. The variants are of a sort which could not possibly depend on a "fortunate restoration".

(l) Tantr. I. 169 = SP. I. 143 = *v* II. 116 = Pa. I. 103 = Pūrṇ. I. 396. Pada c: SP. *β* alone reads *sneho,* "affection", for *guhyaṁ,* "a secret", of all other Sanskrit versions, including SP. *a* and *v*. But Pa. translates a Sanskrit word which must have been *sneho* or a synonym thereof, and cannot have been *guhyaṁ*: "die Freundschaft bis zu den Verleumdern" (Schulthess). Apparently, then, SP. *β* and Pa. go back to one "archetype", while SP. *a*, *v*, Tantr., etc., go back to another.

(m) Tantr. I. 174 = SP. I. 145 = *v* II. 118 = Pa. I. 106 = Pūrṇ. I. 403. (Cf. Hertel SP. LVI f.) In pada c: Tantr. *a* and apparently (so Hertel) SP.'s archetype (with MS. K) read *anugamyo* (most SP. MSS. have unimportant corruptions). But Tantr. *β* and Pūrṇ. have *anukampyo,* to which the corrupt text of *v* (—*kampo*—) evidently goes back. Here then we have Tantr. *β*, Pūrṇ. and *v* forming one group, Tantr. *a* and SP. another.—The lengths to which Hertel allows his preconceived theories to carry him are indicated by his discussion of pada b (l. c., LVII). Here nearly all MSS. of SP., including K, the best MS. of all, and B, another MS. of the *a* recension, read *bhāvyam,* which the meter requires.

A few *a* MSS. read *bhavitavyam,* an obvious and metrically impossible corruption. Hertel finds the evidence of these few corrupt MSS. enough to *absolutely establish* the corruption *bhavitavyam* for the SP. archetype—apparently for no other reason than that the Tantr. MSS. have the same corruption, and Hertel feels the need of making SP. at least as corrupt as the Tantr.!
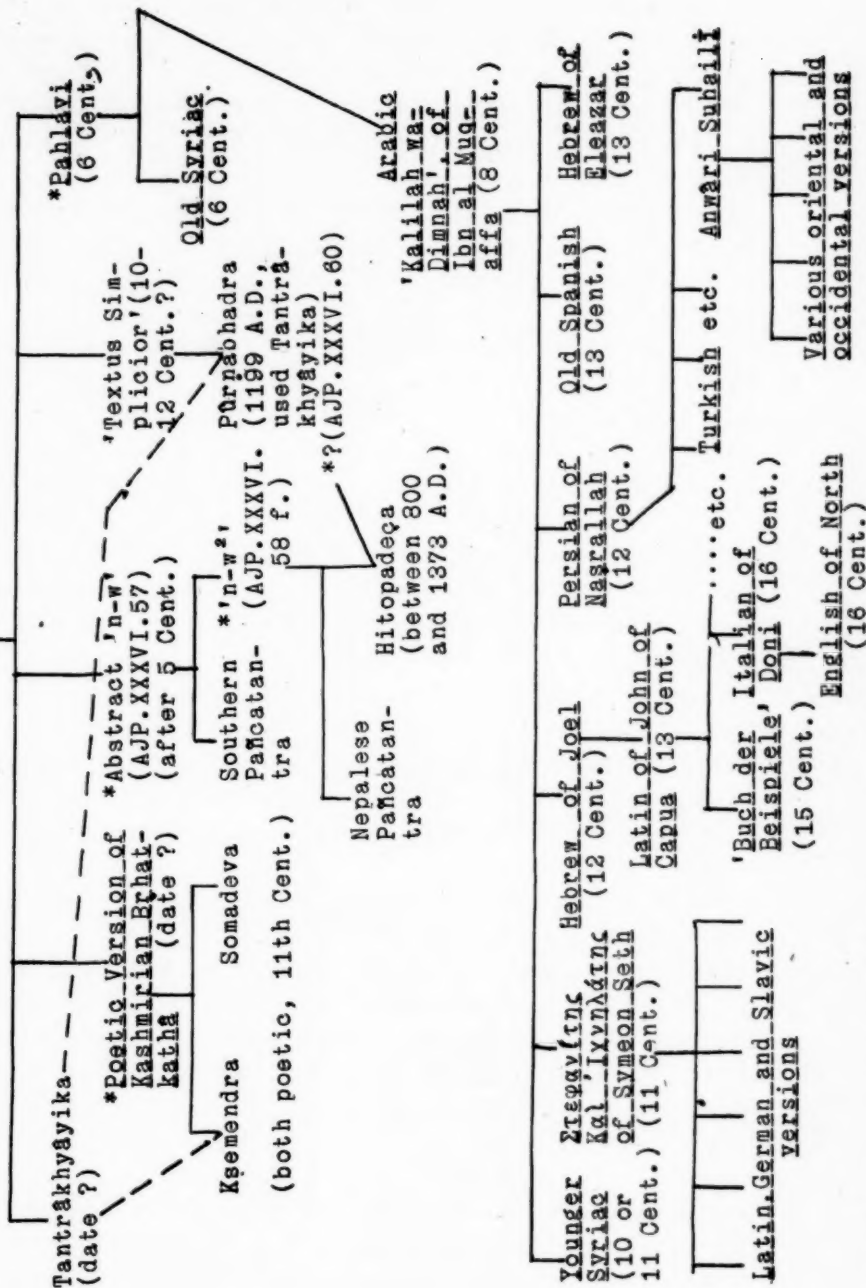
I have chosen only the most clear and striking cases of this sort which I have discovered in the verses of Book I. The same sort of thing occurs repeatedly in smaller variations which could more easily have arisen independently; that is why I have not recorded them—although they include a considerable number of much more marked and important variations than some on which Hertel lays great weight, and in which he sees a deep significance (cf. WZKM. 25. 16 ff., and my comments, above, p. 270 n. 1).

(In Tantr. I. 73 is a clear corruption which Hertel has failed to perceive. It contains fragments of two verses found entire in the other recensions—SP., *v,* Pa., Pūrṇ. and even Som.; SP. I. 65 = *v* II. 48 = Pa. I. 50 = Pūrṇ. I. 232, clearly reproduced by Som. 60. 119, while SP. I. 66 = *v* II. 49 = Pa. I. 51 = Pūrṇ. I. 233, clearly reproduced by Som. 60. 121. Now Tantr. I. 73 ab is SP. I. 65 ab, while Tantr. I. 73 cd is SP. I. 66 cd. Padas cd of SP. I. 65 and ab of SP. I. 66 are accidentally omitted in Tantr. Correct Hertel's statement under Tantr. I. 73 on p. 104 of Tantr. Uebers. I.)
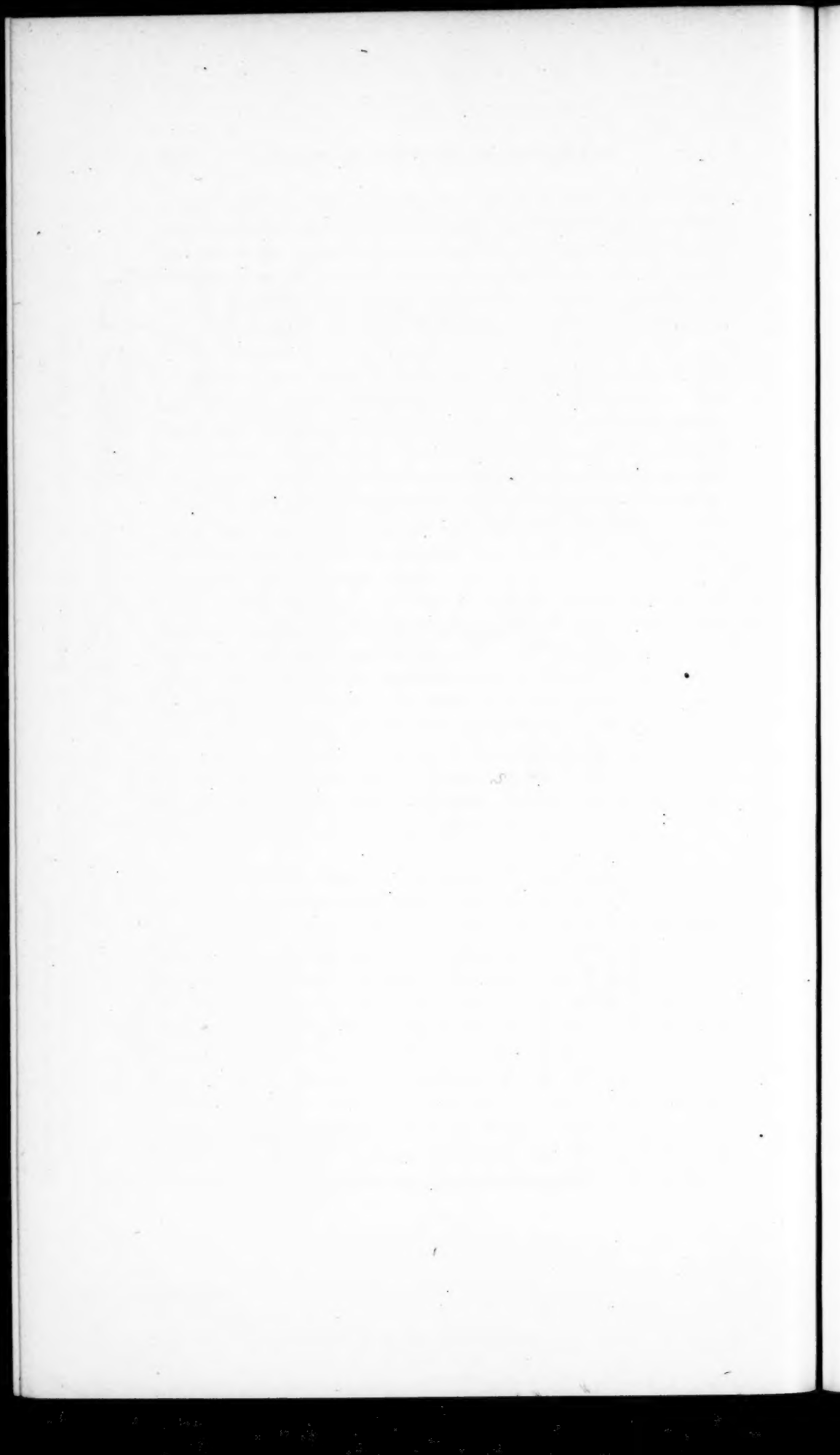
I hope to have shown that there is a total lack of definite proof for Hertel's archetypes "K" and "N–W". On the contrary, as I have hinted, it seems to me that Pahl., Bṛh., Simpl. and n–w are each more closely related to Tantr. than to each other. At present I do not think it is possible to determine more narrowly the relationship of these versions. But in determining the Urpañcatantra version at any one particular point, I hold—in direct opposition to Hertel, whose views rest on the assumed stability of his "K" and "N–W" archetypes—that the agreement of any two or three of the older versions establishes a *prima facie* probability that they contain the original reading—*provided* that the corresponding texts of the other recensions show no agreement among them-

GENEALOGICAL TABLE OF THE OLDER AND MORE IMPORTANT
VERSIONS OF THE PAÑCATANTRA.

*THE ORIGINAL PAÑCATANTRA
(ca. 200 A.D.?)

Tantrākhyāyikā
(date ?)

*Poetic Version of
Kashmirian Bṛhat-
kathā (date ?)

*Abstract 'n-w'
(AJP.XXXVI.57)
(after 5 Cent.?)

'Textus Sim-
plicior' (10-
12 Cent.?)

*Pahlavi
(6 Cent.)

Old Syriac
(6 Cent.)

Arabic
'Kalilah wa-
Dimnah' of
Ibn al Muq-
affa (8 Cent.)

Kṣemendra   Somadeva
(both poetic, 11th Cent.)

Southern
Pañcatan-
tra

*'n-w²'
(AJP.XXXVI.
58 f.)

Pūrṇabhadra
(1199 A.D.,
used Tantrā-
khyāyika)
*?(AJP.XXXVI.60)

Nepalese
Pañcatan-
tra

Hitopadeça
(between 800
and 1373 A.D.)

Younger   Στεφανίτης   Hebrew of Joel   Persian of   Old Spanish   Hebrew of
Syriac   Καὶ Ἰχνηλάτης (12 Cent.)   Nasrallah   (13 Cent.)   Eleazar
(10 or   of Symeon Seth   (12 Cent.)   (13 Cent.)
11 Cent.) (11 Cent.)

Latin of John of
Capua (13 Cent.)

·····etc.   Turkish etc.   Anwari Suhailī

'Buch der   Italian of
Beispiele'   Doni (16 Cent.)
(15 Cent.)

English of North
(16 Cent.)

Latin, German and Slavic
versions

Various oriental and
occidental versions

* Indicates non-extant versions. Translations into other languages than Sanskrit are *underscored*. — — —
indicates influence in minor degree.

selves. *Nota bene*: a *prima facie* probability only; and *only* in case the disagreeing versions are divergent among themselves. Many of Hertel's reconstructions of the Urpañc. are vitiated by his unwarranted assumption that the evidence of Tantr. alone is of at least equal weight with the united evidence of *all* the other versions; nay, he sometimes thinks Tantr. α alone can outweigh Tantr. β with all the other versions, since he believes that Tantr. β is contaminated from a " K " codex. I believe that Tantr. α and β together are of not much, if any, more weight in determining the original Pañcatantra than any other of the older versions.

My opinion of the relation of the older Pañcatantra versions is indicated in tabular form by the following "genealogical table", which I would substitute for Hertel's "Stammbaum" in so far as it is inconsistent therewith. Of course it should be understood that my table is only tentative. Especially is this true on the negative side. That is, I regard it as quite possible that closer relations may yet be proved to exist between some versions, between which the table indicates no closer relation, simply because no such relation has yet been *proved*. The relations indicated positively by my table I regard as so well established that it is unlikely that future investigations will upset them.

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II.—THE -σσ- FORMS IN HOMER.

As is no doubt well known to all who have looked closely into Homer, the scansion of the two epics is graded in a rather curious way. The metre of the speeches [1] is much less exact than that of the narrative proper; and the speeches of B [2] etc. and of the Odyssey go farther in this way than do the speeches of the Iliad* [2]. The narrative of the Iliad* is stricter than the narrative of B [2] etc. or of the Odyssey; and within the Iliad itself the strictest scansion is given by the ἀριστεία in Λ. The next best is Π; E and P, while excellent in parts, do not approach these two. Tested metrically, the books NΞO are remarkably lawless; in narrative this style is a new departure, analogous to ικλμ in the speeches. In the rest of the Iliad* (taken in bulk) the narrative, though showing a downward tendency, still differs from the Odyssey.

Among the changes in the verse, four stand out most clearly. The lines are such that they need to be recited faster; verse-pause becomes conventional, not functional; ictus is less emphasised; and there is a growing disregard of gratuitous spondees. The last point is illustrated by the distribution of the temporal augment and of scansions like καλ|όs etc.; it can be seen too, though perhaps not so clearly, in the handling of the -σσ- forms which arise from dentals and are preceded by a short vowel. Let us first look at the gratuitous spondees (within the line) given by this kind of -σσ- *in arsi*:

(*a*) Narrative—

νείκεσσε Γ 38, Δ 336, 368, Z 325, H 161, Ω 29, ρ 215, 374; ὥπλισσε Λ 741.

ἀάσατο Λ 340, ἠγάσσατο Γ 181; ληίσσατο Σ 28, ὡπλίσσατο Λ 86, β 20; ? ἐρρύσατο α 6, ἐφράσσατο Ω 352.

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[1] See Cl. Q. 1908, April, pp. 94 seqq., and 1912, Jan. and April; and Am. Journ. of Phil. 1913, pp. 43 seqq.

[2] The books B 484-end, Θ, I, K, Ψ, and Ω are tabled apart as "B [2] etc."; the rest of the Iliad is called "the Iliad without B [2] etc." or for short "the Iliad*".

συμφράσσατο Α 537, ο 202, φράσσατο Ψ 453; πρόσσοθεν Ψ
    533.

ἐρύσσαι Φ 175? ὀπίσσω Φ 30? νεμέσσα φ 147.

τανύσσειέν τ' σ 92; ἐρίσσειαν Ο 284, νεμέσσηθεν Β 223, α 119;
    ἐρύσσασθαι Φ 176, φ 125, μαχέσσασθαι Ο 633 (Ρ 604).

μέλλον ἀπέσσεσθαι Ρ 278; ἔσσεσθ' Ο 613; (ἔννυσθαι ξ 522).

ὤμων μεσσηγύς Ε 41, 57; Θ 259, Λ 448, Π 807, χ 93;

μεσσηγύς Ε 769, Ζ 4, Θ 46, Ν 33, Ψ 521, Ω 78, δ 845, ο 528,
    χ 341, 459. ἀνδρῶν τρεσσάντων Ξ 522.

περὶ μέσσῳ Ν 534; μέσσος Γ 78=Η 56, Η 277, 384=417,
    ε 487, θ 66=473; (Λ 167, Π 412=Υ 387, 475).

ὅσσος Β 845, Ρ 261, η 108; τόσσος Γ 12, Δ 430, Μ 338, Ξ
    150, Ψ 522, 847.

κύσσαι ω 236; ζέσσεν Σ 349; τρέσσαι (-αν) δ' Ν 515, ζ 138;
    (εἴσω Η 270, η 13, θ 290),

(*b*) Speeches—

ἐξείνισσε Γ 207, τ 194, ω 271; (ἐ)ξείνισσε Γ 232, ω 266,
    288; ξείνισσε Ζ 174.

ἀάσσατο Τ 95? ἠράσσατο Υ 223, λ 238, ὠμόσσαμεν Υ 313;
    ληΐσσατο α 398, ὡπλίσσατο ι 291, 311, 344, κ 116.

? ἐῤῥύσατο Υ 194, ἐφράσσατο δ 529, συμφράσσατο Α 540,
    δ 462; αἰδέσσεται Χ 419, ξ 388, θαυμάσσεται Σ 467.

ἰλάσσομ' γ 419, ληΐσσομαι Ψ 357, ξεινίσσομεν η 190, ὁρμίσ-
    σομεν Ξ 77, πεμπάσσεται δ 412, τοξάσσεται χ 72;

φράσσομαι ε 188, ω 217, ἐπιφράσσετ' ο 444, συμφράσσομαι
    Ι 374.

ὀπίσσω Γ 160, καὶ ἐλάσσῃ Ε 236, ἐρύσσῃς Ε 110, λοέσσας
    Ψ 282;

λοέσσαι τε τ 320, ἐρίσσειε Γ 223, ο 321, τ 286, ἐρύσσωσ'
    ρ 479.

νεμεσσητόν Γ 410, Ι 523, Ξ 336, Τ 182, Ω 463, χ 59, 489;
    νεμεσσηθείς Ο 211, 227; μαχεσσαίμην Ν 118;

ἀρεσσάσθω θ 396; ἀνύσσεσθαι π 373, ἐρύσσασθαι Σ 174,
    μαχέσσασθαι (Ε 483, β 245), σ 39, ὀνόσσεσθαι ε 379,
    (ἀνδρ' ἀπαρέσσασθαι Τ 183); ἐφέσσεσθαι Ι 455, ἀπέσ-
    σεσθαι σ 146, ἀπέσσονται υ 155;

δάσσασθαι υ 216, ἔσσονται Ζ 353, Χ 255, 266 (?), 489,
    λ 137, ψ 284, ἔσσεσθαι Ζ 339, Μ 324, χάσσασθαι
    Μ 172, (ἴσασιν Ψ 312, β 283); μεσσηγύς η 195, χ 442;
    ποσσῆμαρ Ω 657.

ἐν μέσσῳ μ 20; μέσσος Γ 416, λ 157, μ 80, 93, 443.

ὄσσος Α 186, Ζ 454, Κ (214), 418, Λ 657, Ρ 23, Ξ 94,
(Τ 230), Φ 488, Ω 544, η 68, θ 222, κ 45, λ 329, 518,
(ξ 93, π 122, τ 130), χ 168; τόσσος Χ 423, α 248,
ι 243, 265, μ 123, ρ 407; τοσσοίδ' Ξ 94, Υ 357 ? 359 ?
(εἴσω Σ 282, κ 91).

ἔσσω -ει π 79, ρ 550, ᵉ57, φ 339, ἔσσαι ξ 154, ἔσσας ξ 396.
ζέσσεν κ 360.

In narrative the most surprising instance is νείκεσσε which
even in the speeches has no true parallel (ξείνισσε is legitimised
by the ξ-) ; it is interesting that except in Ω 29 the word comes
only in speech-introductions, and has, except in ρ 374 (and
χ 225), a metrical concurrent in ἐνένιπε. So too in late narra-
tive ὥπλισσε replaces ἐκύκησε (Λ 678).

The types ληΐσσατο and (within the line) φράσσατο seem
peculiar to late work; the simile in Λ 84–90 is in all points
Odyssean. As can be seen from the table, the use is character-
istic of the speeches, where there are 24 or 26 examples ; γ 419,
λ 238, and ο 444 are the most extreme cases. The scansion
ἠγάσσατο (Γ 181) probably stands alone in narrative; Λ 340
looks like a hybrid from (φυγέμεν) καὶ ἀάσσατο (κηρὶ πεδηθείς)
and ἀάσσατο δὲ (μέγα θυμῷ).

Words like ὀπίσω hardly ever take middle ictus except at
the end of the line. In narrative there is no clear exception
until φ 147; in Φ 175 παχείῃ χειρὶ ἐρύσσαι will go in, and in Φ 30
χεῖρας ὀπίσσω is possible (cf. εἴσω Π 340). Even in the speeches
bacchiacs of this kind nearly always close the verse; there are
only four clear exceptions, of which Ε 110 and Ε 236 are the
most puzzling, since ἐρύσσεαι ὀξύν and ἐλάσῃ τ' ἐριαύχενας would
a priori seem to be the natural scansions.

A form such as τανύσειε is obviously preferable to τανύσσειε
which creates a spondee and also gives a half-open antispast.
The artificial ictus is found only once in narrative (σ 92) and
five times in the speeches ; τ 320 shows that the scansion was
at last naturalised, and in ρ 479 we actually get a shifted ex-
ample. When the forms end with a true open syllable, as in
ὀνόσαιτο or ἐρύσασα, they never take artificial ictus. Closed
antispasts occur thrice in narrative and seven times in the
speeches, and infinitives like ἐρύσσασθαι must also be added.

Of these there are in narrative perhaps 3; in Φ 176=φ 125 it is a puzzle why λιλαιόμενος ἐρύσασθαι was not preferred. In the speeches there are 7 instances (beside ν 155); in this as in many other points the speeches of the Odyssey go to the greatest extreme.

Infinitives like ἔσσεσθαι or χάσσασθαι seem to have only one certain instance in narrative; in the speeches there are four, beside 6 occurrences of ἔσσονται. The similar scansion μεσσηγύς is regular only in such a phrase as ὤμων μεσσηγύς, where ὦμοι and μεσηγύς together must give one spondee, but did not originally give two (ὠμόφι μεσσ.). The remaining trisyllable is legitimised by the τρ- (τρεσσάντων ἀνέρων would be quite regular).

The scansion of ὅσος is complicated by the fact that time, measure etc. were originally thrown into a primitive kind of generalising clause. Owing to the special sense, the subordinating words which introduced such clauses needed correction with τε—at least it is a curious thing that in narrative the instances of ὡς introducing a comparison are either shifts from the back-leaning ὥς (ε 371, ζ 20) or else metrical replacements of ὥς τε etc.; thus, ὡς δέ=ὡς τε, and ὡς δ' ὅτε may be a hybrid from ὡς δ' ὅτε τε and ἠΰτε. Since generalising τε must not stand before any other particle, similes or other clauses of the kind cannot easily be brought in with δέ or γάρ when relatives are used—ὥς τε δέ gives the wrong order (ὥς τε γάρ only in a speech, B 289) and ὡς δέ τε gives the wrong sense. So clauses of this kind must either be brought in with asyndeton, just as we see the ἠΰτε similes in the text, or else some special device must be employed to get the conjunction before the corrected subordinant. This is done by ὡς δ' ὅτε τε (written as ὡς δ' ὁπότε, ὡς δ' ὅταν, etc.) or by οἱ δ' (τοὺς δ') ὥς τ' etc.; the frequency of the latter has been shown by Professor Gildersleeve, and illustrates very well the point suggested here. Further, a generalising clause ought to precede its apodosis, so that the short scansion ὅσον τ' could only have been obtained by holding back the sense-pause and asyndeton to the weak caesura. Spondaic ὅσσον τ' was therefore always probable, and seems at last to have become normal—E 770, (860), Ξ 148, Π 589, Ψ 517, (845), Ω 317, ε 249, (484), θ 124.

Beside ὅσσος the one other common spondaic form is μέσσος. If μέσατος is possible, the early examples are easy to explain

(κὰμ μέσατον πεδίον, etc.), as is also the frequency of μέσσ|ος which with other examples of -σσ|- must now be considered:

(*a*) Narrative:

μέσσ|ος Γ 266, 341, Δ 79, 212, Κ 265, Λ 172, 413, Μ 209,
    Ο 357, 635, Σ 507, 569, Τ 77, 249, Υ 15, Ω 162, β 150,
    δ 844, ε 326, σ 89, ω 441.

type ὀχλίσσειαν Β 282, Δ 498, Κ 571, Μ 448, Ο 575, Τ 385
    Χ 404, Ψ 55, ν 184, π 459, σ 94, τ 391;
    φράσσαντο Ο 671, φ 222; πρόσσω Σ 250, ω 452.

other words κάπνισσαν Β 399, (κὰδ δ' εἶσ' Γ 382), παρέτ-
    ρεσσαν δέ Ε 295, ἀόλλισσαν Ζ 287, ? ἀήθεσσον Κ 493.

μεσσηγύ Λ 573 = Ο 316, ἀκοντίσσαι Ν 559, 585, Π 359,
    ποσσὶ κραιπνοῖσι Ψ 749, μέσσαυλον Ω 29.

ἐφοπλίσσαι τ 419, δάσσαντό τε τ 423, νείκεσσεν δ' χ 225.

ὅσσ|ος Β 681, Κ 351, Ο 673 ἠδ' ὅσσοι, ε 400? τόσσ|ος
    Ξ 396, 398, Σ 378, Ω 230, χ 145.

also ἔσσ⁵|αντο Ξ 350, ῥ' ἔσσαντο Ξ 383, ω 467, 500; εἴρυσ-
    σέν τε Γ 373 = Σ 165; στήθεσσιν Α 189, σπέσσι . . . .
    (Thus—σπέσσι α 15 ε 155, ψ 335.)
    (-εσσ|ι Λ 162, Μ 382, Π 704, [Ρ 396?], [δ 116?], ν 432).

(*b*) Speeches:

μέσσ|ος Γ 69, 90, Ζ 181, Ν 312, Τ 173, Υ 245, Ψ 241,
    δ 281, 413.

type ὀχλίσσειε Α 216, Δ 324, Ε 216, Ζ 270, Η 449, Θ 143,
    Ι 426, Ν 741, Π 545, 559, Ρ 327, Σ 276, Τ 26, Χ 489,
    Ω 263, 567, β 295, δ 535, (ε 122 ἠγάασθε), ζ 57, 69, η
    306, ι 242, λ 411, ρ 268, χ 78, 134, ψ 188, ω 360.

φράσσονται β 367, φράσσαντο κ 453, ω 391, σπάσσασθε χ
    74; σβέσσαι Ι 678, Π 621; πρόσσω Α 343, Γ 109, Λ 615.

other words ἀεικέσσι Β 264, Μεσσηίδος Ζ 457, αἴδεσσαι
    Ι 640, ἔσσεσθαι Ο 292, ξ 176, π 311, (Λ 444? Χ 332?)

χάσσονται Ν 153, μεσσηγύ Υ 370, ἑταιρίσσαι Ω 335, ὅπλισ-
    σον β 289, ἄασάν μ' κ 68, πάσσασθαι κ 384, μέσσαυλον
    κ 435;

ὅσσ|ος Ι 160, Ρ 172, (δ 356?), 723, ζ 294, θ 102, 252,
    ι 473, λ 25, μ 181? π 236, 290, τ 9, ἠδ' ὅσσ|ος Ι 161,
    Ψ 891, α 247, ν 241, π 124, οὔθ' ὅσσ|ος φ 346, 347,
    ἀλλ' ὅσσος Υ 360;

τόσσ|ος Ζ 335, 450, Ρ 20, 253, Φ 321, Ω 670, ι 324, 499,
   τ 169, 221, χ 50? ω 276; τοσσόσδε Χ 41, γ 205, δ 665,
   ε 100, φ 253; τοσσοῦτον ξ 99, φ 250, 402, (τοσσαῦτ᾽
   ἔτεα Β 328, ἢ τοσσοῦτον θ 203).

also εὖ δάσσαντο Α 368, σπέσσι α 73, δ 403, ι 30, 114,
   διήρεσσ᾽ ξ 351.

(εἴσ|ω Φ 125; ἔσω only in Β² etc. and Od.—Ω 155, 184,
   199, η 50, λ 579, σ 49, ψ 24).

(ἴσασι θ 559, 560, λ 122, 124, ξ 89, ψ 269, 271, ω 188).

(-εσσ|ι Ζ 362, Κ 441, [Λ 319?], [Σ 123?], β 47 τοίσδεσσιν,
   166, η 59, ξ 238).

Of the narrative instances here no less than 21, or roughly
a third, are due to μέσσος. Twelve more are words like ὀχλίσ-
σειαν, where the false length is not easy to avoid; but it is not
so early as πνείοντες, and perhaps came in from the speeches
where the forms are more needed; the aorist εἰρύσσασθαι (-αιτο
etc., narr. π 459, speeches Α 216, Θ 143, Ρ 327, Σ 276) is the
most difficult. Taking the remainder of the list from πρόσσω
to τόσσος, we find at most 26 instances in narrative, as against
at least 53 in the speeches. Among the exceptional words
three are important, for if they are possible in early narrative,
it will be difficult to get a simple view of the -σσ- forms in
general. In Ε 295 there is, beside the spondee, a displaced δέ; [1]
the phrase is a shift from ἐρώησαν δέ οἱ. In Π 359 ἀκοντίζων
seems conceivable, and its replacement by an infinitive may be
the origin of the curious use which is fixed in Ν 559 (cf. τ 419).
Λ 573 is harder; beside the spondee, which is not more than
formally justified by καί, there is the fixed -ειν, [2] and χρόα not
dovetailed. All these points are quite in keeping with the
style of ΜΝΞΟ, whence the line may have been transferred. [3]

Apart from these instances there seems to be no real objec-
tion to the view that in the earliest epic period the -σσ- forms
represent, not the preservation of an original double conso-

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[1] Il.* (narr.)—Β 86, 398, Γ 348, Δ 154, Ε 856, Ζ 311, Η 259, 267, 269,
Ν 175, 366, 608, Ξ 497, Ο 452, 464, 550, 581, Π 163, 341 *bis*, 348, Ρ 44, 373,
Τ 280, 382, Υ 80, Φ 244. In Π 341 ὑπὸ δ᾽ ἤριπε is possible, but *sequitur
varam vibia.*

[2] Il.* (narr.)—Θ 319, Ι 184? Λ 340? 573, Ν 387, Μ 111, 301, 337, 341,
Ξ 424, Ο 316, Χ 413.

[3] The sense would end with πόλλ᾽ ἄρ᾽ ἐνὶ μεγάλῳ σάκεϊ πρόσω ὁρμενα πῆκτο.

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nant, but merely the scanning of -σ- forms under ictus.   The
effect of ictus varied according to the nature of the syllable.
When short *a*, *ε* or *o* were followed by a liquid or nasal (or
sibilant), ictus had an effect rather like that of the English
stress-accent, and produced doubling. · On short *ι* or *v* the
effect was different; the vowels themselves were lengthened
by the ictus, as is shown by ὕδωρ where doubling is improbable,
and by ἱερός, etc., where there is nothing to double.   The
placing of short syllables under ictus was never limited by
metrical necessity; it was motived by metrical convenience,
and was used freely with one great restriction—it should not
give a gratuitous spondee within the line.   Thus ἀνήρ and ὕδωρ,
iambi which put no pressure on the verse, are often used with
ictus of the first syllable, but only where a spondee is legiti-
mate, viz. at the end of the line; in the narrative of the Iliad*
there is no certain exception to this (M 382 admits ἀνὴρ ἔχοι).
So too closed tribrachs (ἀνέρες -ας, οὔρεος, etc.) are normal;
indeed, owing to the fact that the resulting forms do not give
a spondee, the treatment of tribrachs becomes extraordinarily
free—e. g. εἴρυσε -αν and ἔρυσσε as well as ἔρυσεν -αν, and οὐλοός
(Λ 62) and ὀλονός as well as ὀλοός.   Closed antispasts ('Απόλ-
λωνος) are regular, and even the preference for -οιο is enough
to bring in the artificial ictus (Οὐλύμποιο).   But 'Απόλλ|ων is
never found, nor scansions like οὔρεσφ|ιν, and Οὐλύμπῳ is seen
only in Φ 389—this line, which in four feet denies five princi-
ples, is one of the finest shifts in Homer.

In the earliest period, then, it seems that the stress of the
arsis could lengthen *ι* and *v* and could double a consonant.
Doubling was easiest for -σ-, next for -ρ-, and then for -λ-,
-μ- and -ν-; next comes -π-, no doubt pronounced as an ex-
plosive; the other consonants are not so favourable, except
-ϝ-, which was perhaps not doubled but made into a diphthong.

The stressed pronunciation of the arsis seems to be only
primitive.   Later it weakened or disappeared, and a fine " epic "
mystery is the result.   The -σσ- forms, like the ὁππότε, ὅττι[1]

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[1] The appearance of ὅππως etc. in inscriptions cannot be made to
prove anything.   That epic metrical forms could come into common
speech is clearly shown by the Attic use of πουλύπους.   The words
ὁπότε, ὅπως, etc., seem to be nothing more than ποτε, πως, etc., com-
pounded with the ιο- stem.

group, are petrified, and they are used with increasing freedom, spondee or no spondee, arsis or thesis. The other double consonants are swept away (except ἐννοσίγαιος), and vowel-length, originally proper to ι and υ, is substituted; this gives rise to new scansions like κείατο, ἐτελείετο and μαχειόμενος which in the old verse are impossible, since they have no consonant to double. Finally there are prophetic glimpses of Epichares walking to Marathon; for in the case of some words, undimmed by primitive epic procedure (ἐπεὶ δή, ἐπίτονος) the text justly rejects gemination—doubling had gone out before ἐπεὶ| δή came in—and humanely shrinks from vowel-length.

The forms with -σ- which is never doubled may perhaps be urged against the above position. Those where ictus of the -σ- syllable would give a bad scansion (ἀσάμινθος, ἥμισυ etc.) call for no special notice; the rest fall under six heads : a) δασύς and θρασύς, b) δύνασαι and ὄνοσαι, c) 3rd plurals like ἴσαν and φάσαν, d) adjs. in -όσυνος and nouns in -οσύνη, also πίσυνος, e) nouns like νέμεσις, d) πέσον -ε etc. Of these, δασύς comes only once (ξ 51) and θρασύς, owing to the θρ-, is not likely to be ictuated (θαρσαλέος and θάρσυνος are used instead). The rest seem at first to illustrate the objection that -σ- is never doubled unless it represents an original double consonant. The argument really cuts both ways. Whenever -σ- is certainly primitive, it must come from two consonants ; when it is not primitive, then it represents what in the epic period may have been -τ- (πέσον) or -θ- (πίσυνος) or no consonant at all (μέμασαν: μεμάασι). The fact is that an original double consonant does not always lead to -σσ- scansion—*e. g.* ἴσαν ('knew') is never lengthened; and conversely a modern or reconstructed -σ- (which must be true single -σ-) can be metrically doubled, as perhaps in ἐπῆσαν (τ 445), and certainly in νεμέσσι (Z 335). Both of these scansions are anything but early, and suggest that the use of -σαν and the change from -τις to -σις arose in the later epic period (cf. φάτις :πάρφασις and παραίφασις), the change from πέτον to πέσον can hardly have come within the epic period at all, otherwise πέσσε δέ would have been almost inevitable in B² etc. and in the Odyssey.

. Among the instances of spondiac -σσ- those given by 'Οδυσσεύς have so far been omitted and must now be noticed. The

scansion of the name is very puzzling. Whatever 'Οδυσσεύς may be or mean, it is quite clear that the -σσ- is only metrical. In the oldest narrative in which the name is found (Λ 310–488), it shows -σσ- not more than four times, and three of these are speech-introductions (Λ 312, 449; and 346), while there are no less than eight examples with -σ- (Λ 335, 396, 401. 439; 419, 473, 482; 459) which all give true scansion and so are not likely to be mere recitational shiftings, which would scan like the Odyssey, where out of 37 occurrences of 'Οδυσσεύς (narrative) only 13 give true scansion—ε 387, ζ 113, η 43, θ 531, ξ 523, ο 485, τ 413, 452(?), φ 34(ἄορ), 129, 225, χ 381, ψ 306.

The -σσ- then seems to be purely metrical; but this conclusion has the rather awkward consequence that out of 595 occurrences in the two epics the form gives 85 avoidable spondees, of which 67 [1] are in narrative, and 35 of these are scansions otherwise quite unparalleled. The probable solution of this little mystery is no doubt foreseen by the reader, or else he can easily guess it from the following table:

The table seems to show that 'Αχιλλεύς and 'Οδυσσεύς have affected each other metrically. The convenience of scansional interchange was very great, and brought about a twofold result. The real forms 'Αχιλλῆος etc. lead to the free use of 'Οδυσσῆος etc., and conversely the real forms 'Οδυσσεύς -ῆα etc. lead to the free use of 'Αχιλεύς -ῆα etc.; so that both the artificial ictus of 'Οδυσῆος and the false shortening of 'Αχιλλεύς are felt to be perfectly epic. That 'Αχιλλεύς is the original form of the name is quite clear; the resulting antispasts have standing scansions on either side of the caesura ('Αχιλλῆα πτολίπορθον and 'Αχιλλῆα ῥηξήνορα); the former of these would be most improbable, and the latter downright impossible, if 'Αχιλλεύς were the artificial ictus of 'Αχιλεύς. Even if there had been a genuine form 'Αχιλεύς side by side with 'Αχιλλεύς, the scansion 'Αχιλλῆα or -ι would only have appeared as a rare device after the 3d trochaic in late work (artificial

[1] Avoidable spondees from 'Οδυσσεύς, narrative—B 631, 636, E 519, η 14, φ 227, ψ 320; Λ 346, ε 81, 149, ζ 212, θ 144, 517, ν 117, ρ 301, σ 356, τ 473, χ 163, ψ 153, ω 494, 541; B 335, Θ 222, I 218, K 271, Λ 5. 806, a 129. β 2, 35, 394, 415, γ 64, 398, δ 625, δ 799, ε 198, 297, 406, θ 75, ο 2, 59, 63, 554, π 48, 53, ρ 3, 34, 167, 292, 299, σ 311, υ 248, 283, 290, φ 189, 432, χ 147, 238; E 674, I 180, ε 233 ζ 14, 248, θ 9, ν 73, ξ 447, υ 281.

antispasts are eventually so used sometimes), but it would never have been dovetailed in the strong caesura. What really happened is obvious; the two names were made artificially interchangeable. It is interesting that the merely metrical 'Αχιλεύς never quite competes with the genuinely existent 'Οδυσεύς, as the reader can see by examining the table.

{'Οδυσσεύς / 'Αχιλλεύς	NARRATIVE.			SPEECHES.		
	Iliad*.	B² etc.	Odyssey.	Iliad*.	B² etc.	Odyssey.
'Οδυσσεύς...	10+ 9¹	12+10	84+97	7	1	60
Displaced	1—	2 —	2+ 1	1	—	10
'Αχιλλεύς ..	48+35	25+17	— —	23	12	2
Displaced	1+ 1	4+ 3	— —	1	1	0
'Οδυσεύς ...	11 —	9+ 3	30+ 7	2	1	31
'Αχιλεύς ...	12+ 2	9+ 2	— —	8	2	—
'Οδυσσεῦ ...				2	5	24
'Αχιλλεῦ...				9	7	6
'Οδυσεῦ				2	3	11
'Αχιλεῦ				6	6	1
'Οδυσσῆα...	— 1	— —	7+ 6	—	—	5
'Αχιλλῆα ..	4 —	— —	— —	2	1	—
'Οδυσῆα....	5+ 1	1+ 1	33+ 9	—	1	14
'Αχιλῆα ...	11+ 1	1+ 2	— —	10	1	—
'Οδυσσῆος ..	2+ 1	3 —	29+ 3	1	—	—
'Αχιλλῆος ..	13 —	2 —	1—	9	3	2
'Οδυσῆος ...	1 —	1 —	22+ 3	2	1	35
'Αχιλῆος ...	9+ 1	1 —	— —	10	6	2
'Οδυσσῆϊ ...	1 —	1 —	6+ 1	—	—	1
'Αχιλλῆϊ...	1 —	— —	— —	4	6	0
'Οδυσῆϊ	1 —	2 —	14+ 1	3	1	10
'Αχιλῆϊ	10+ 2	1 —	— —	15	6	—

[1] The figures added with + give the occurrences in introductions and resumptions of speeches.

The introductions and resumptions of speeches are in this point, as in most others, scansionally very difficult; if they are neglected, the figures give a quite clear result.

The datives in -εσσι (types βόεσσι, Τρώεσσι, ἐπέεσσι, etc., not types πελέκεσσι, ὄεσσι, ἔπεσσι, στήθεσσι) show plainly enough the difference between metrical -σσ- and -σσ- that was normally pronounced as a double consonant. True -σσ- was not shortened—at least we must think it an unlikely chance that in narrative as against 335 instances of -εσσι there are only 4

instances of -εσι (K 486, Ψ 468, Φ 191, ο 557), which can be reduced, but cannot easily be much increased; if -εσι had been a genuine form and not a false shortening, scansions like χείρεσι would have been very common. The scansion -εσσ|ι is very rare indeed; but this is not because the -εσσ- is not a true long syllable, but because the form was avoided whenever it gave a gratuitous spondee as can be seen from the following:

Dat. in -εσσι.	Iliad (not B² etc.	B² etc.	Odyssey.
NARRATIVE: Regular .... Avoidable spondees.	117+56 Τρώεσσι 28[1]	26+ 6 Τρώεσσι 8[1]	72+1 Τρώεσσι 22[1]
SPEECHES: Regular .... Avoidable spondees.	91+55 Τρώεσσι 28[2]	17+10 Τρώεσσι 20[2]	126+8 Τρώεσσ. 53[2]

The table shows how the irregular -εσσι encroaches, until in the speeches of B² etc. and of the Odyssey it may be taken as normal. Four words, ἄνδρεσσι, νήεσσι, πάντεσσι, and χείρεσσι, are worth looking at separately. The first no doubt represents ἀνέρεσσι in earlier styles; by and by it would become ἄνδρεσσι, but this form can only be proved for very late work indeed (K 441, speech). The dative νήεσσι has 8 narrative instances, of which two (Λ 22 ἐνὶ νηυσίν, 311 δή ῥ᾽ ἐνὶ νηυσί) could be removed, leaving only A 71, B 688, 771, M 112, O 603,

[1] Narr.: A 71, B 688, 771, Γ 80, 271, E 546? 559? Z 397? Θ 116, 544? K (486?), 529, Λ 22? 162, 311? M 27? 112, 135, 303, 382? O 311? 603, Π 352, 386 (?), 704, 801. P 308? 569, 620, 745, T 18? 252, Σ 521, (Υ 468), Φ 285? Ψ (191), 597, 829, Ω 798, β 395, (δ 116?), ε 256 (?), 394 (?), θ 21, 528, ν 432, ξ 10, 21, 448? (ο 557), π 161, ρ 214, σ 35, τ 467, υ 175, φ 3, 81, 147, 379, χ 131, 247, 332? ω 410.

[2] Speeches (A 288), B 175, Γ 283, 367, Δ 239, E (486), 874? Z 492? H 229, Θ 166, 380, (527), I 121, 347, 428, 488, 528, 674, 691, K 173, 214, 245, 279, 310, 342? 397, 441, N 320, 452? 633? 742, 832, Ξ 246, O 229, Π (95), 832, P 40, 451, Σ 91? (123), 328? T 59, 100, 104, 109, 270? Υ 137? 242! 369, X 386, Ψ 248, 554, (671), Ω 408, α 91, 358, β 166, 175, γ 131, 381 (?), ε 344, 348, 473, ζ 267, η 59, θ 167, 538, ι 487, κ 120, 211, 253, λ 352, 399, 406, 431 (!), μ 215, 337, ν 292, 301, 302, 317, 390, 397, ξ 238, 242, 260 (?), 312, ο 178, 315, π 103, 444, ρ 159, 429 (?), 450, σ 317, τ 182, 355, 576, υ 48, φ 197, 235, 352, χ 199, ψ 268, ω 109, 168, 427; τοίσδεσσι K 462, β 165, ν 258, and β 47.

Ψ 829, against 44 occurrences of νηυσί. In the speeches there are about 130 instances of νηυσί, but the increase is not proportional to that of νήεσσι, which is used 29 times, and this too though the speeches have a third resource in νέεσσι (Γ 46, 240, 444, I 602? K 342? Ξ 51, O 722, T 135, ξ 230; somehow or other a curious attempt has heen made to remove the form— Δ 181, E 641, Π 95) ; νέεσσι was not available in narrative, or at least it is found only in the style in which transferred speech-scansions are most noticeably plentiful (N 333, O 409, 414). Non-spondaic πᾶσι occurs in narrative 49 times, as against 6 instances of πάντεσσι and 6 other spondees (B 579, K 20, O 103, 280, Π 159, φ 147) ; in the speeches there are 69 regular scansions of πᾶσι, against 24 instances of πάντεσσι and 16 other spondees (A 288, Δ 289, M 242, P 671. Ψ 671, 787, α 71, 91, γ 59? δ 176, ζ 265, θ 497, κ 518, λ 26, 491, ν 402, τ 550). These two datives give a beautifully clear and unforced illustration of the way in which the verse degenerates. The scansion of the other common word, χερσί, was no doubt disturbed by the existence of χειροῦν, but even here the same drift may perhaps be seen. In narrative χερσί is found about 120 times; χείρεσσι has 20-instances (of these M 27, 382, P 620 are doubtful, and perhaps E 559, T 18, ξ 448, χ 332), and there are 7 other spondees (K 328, M 397, Ξ 176, Ψ 686, 711, α 153, θ 84). In the speeches there are 115 occurrences of χερσί, against 14 of χείρεσσι and 13 other spondees (Ξ 373, Σ 123, Υ 360, X 497, δ 506, ι 108, 416, λ 595, μ 174, 246, σ 335, φ 315, 373) ; that is to say, χείρεσσι and χερσί(ν) are relatively commoner, while χείρεσσιν (=χειροῦν) is relatively rarer.

The -εσσι arising from -εσι is legitimate in στήθεσσι, but giving a gratuitous spondee it is very rare indeed—speech H 135, narr. A 358, Σ 36, (ε 335), and Ψ 131, ω 496; the curious ὅπλοισιν (K 254, 272) must also be considered (ἔντεσσιν). The scansion probably represents -εσφι as a preposition is found with all the instances, βένθεσσι, (ἔντεσσι), τείχεσσι, τεύχεσσι and the regular στήθεσσι (except perhaps Σ 317=Ψ 18).[1] In the speeches -εσσι is fixed, even in thesis (B 264). There is one

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[1] The only regular scansion of a form like στήθεσσι is a dovetail. In narrative there are 2 certain exceptions (Σ 317=Ψ 18), and 4 doubtful (E 513 ἐνὶ δέ, K 9 πύκ' ἐνί, O 322 θέλξεν ἐνί, ν 22 φάτ' ἐνί) ; in the speeches

more exceptional scansion, ἴρισσι (Λ 27), which begins as badly as it ends.

The distribution of the spondees given by irregular ictus must now be considered:

A) ictus-length proper—

ἀνήρ M 382? Ψ 112, speeches ζ 184, μ 77, π 45; ὕδωρ speeches O 37, ε 185;

ὅππη M 48, speeches N 784, a 347, θ 45, ι 457, ξ 517, o 339, π 81, φ 342; ὅππως Ξ 160, Π 113,

Σ 473, o 170, 203, υ 29, speeches A 344, I 681, K 225, 545, Υ 243, Ψ 324, a 270, 295, ξ 329,

τ 298, υ 39, ψ 37; ὁπποῖος Υ 250? a 171, ξ 188, τ 218· ὅττευ χρηίζων, ρ 121;

ἐπεὶ δή Ψ 2, δ 13, θ 452, φ 25, speeches X 379, ω 482; Οὐλύμπῳ Φ 389, speech λ 315;

Οὔλυμπόνδε A 221, Θ 439, speeches A 394. 425, O 133, Υ 142, Φ 438, Ω 104;

Οὔλυμπός τε A 497, E 750, Θ 394, speech T 128; Οὐλύμπου Π 364? Σ 616? speeches Ξ 298? 309?

Also the following:—narrative—δαΐζων Λ 497, πιφαύσκων K 502, Σ 500, ἀν' ἰωχμόν Θ 89, 158, ὀλοιή X 5, γελοίων υ 347, ζευγ-νῦμεν Π 145, μήνιεν B 769, (ἐπιλίγδην P 599, ἀπαμήσειε Σ 34, ἀποέρσειε Φ 329), εἰοικυῖαι Σ 418, ἀφιεῖσαι η 126, γελοίωντες σ 111, υ 390, (κονίοντες Ψ 372, 449, θ 122), ἐξείρυσε Ψ 870, (ἀπενίζοντο K 572), καὶ μείλινα T 361, καὶ πιέμεν σ 3, ἐκ κράατος Ξ 177; ex-ternal—ἀπὸ νύσσης Ψ 758, θ 121, ἅμα νύμφαι ζ 105, ἀνὰ ῥῖνας χ 18, ω 318, καὶ ἀπὸ λέκτροιο ψ 32, ὄφρα λείψαντε Ω 285, o 149.

Speeches:—ἰεῖσαι μ 192, ὀλοιῇσι A 342, ἀάατον (i. e. ἀάαστον?) Ξ 271, ἀποειπών T 35, ἀποέρσῃ Φ 283, ἀναῤῥοιβδεῖ μ 104, ἐπιτέλλω ψ 361, (κονίοντες N 820), κατανεύων ι 490, τριχάικες τ 177, ὤιετο κ 248, κατ' ἀνδρῶν κράατα T 93, σῷ δ' αὐτοῦ κράατι χ 218; ex-ternal—ἵνα μή H 353, ἔτι νῦν o 99, ἐνὶ λέκτρῳ τ 516. Also ὄιω O 298, Φ 533, Ψ 310, τ 215; and ὄιων from ὄις, λ 402, μ 129, ω 112.

there are 10 certain (E 125, I 256, 554, 610, 629, K 90, β 125, π 275, ρ 47, υ 366), and 6 doubtful (A 83, N 732, P 470, Σ 110, ε 222, κ 329).

The treatment of the datives from ἔπος, ὄρος, etc. is well worth analysis, but δηρὸν ἐγὼ κακὰ πάσχον ἐν αἰνῇσι τροχάδεσσι, and had to leave over the other forms.

The most interesting example in narrative is Λ 497. The scansion in itself is not altogether improbable, for in the narrative of the Iliad* closed bacchiacs beginning with a consonant (βαθείης, καὶ ὄγκους, etc.) are the commonest type of spondaic displacement in the second half of the line. Most unfortunately, nothing can be proved from Λ 497, since δηιόων is always possible. The distribution of ὅππως, etc. suggests that these forms are ictuations by analogy with ὁππότε, etc. The earliest example is M 48 ὅππῃ τ' ἰθύσῃ, where the spondee is on all fours with the ὅσσον τ' group discussed above; analogously, since it is needed at the beginning of the line, ὅππως comes in next, and in the speeches is used rather freely, bringing in also ὁπποῖος and perhaps even the extreme scansion ὅττευ. The word Ἄρης should also be noticed, for if it was originally an ictuation, it is petrified in E 518, 594, Ἄρης δ' (τε). The instances of irregular ictus in general go to show that originally the doubling of -ν- and -λ- and even -ρ- was not quite so easy as that of -σ-, and therefore the petrifaction of these scansions is not so early as that of the -σσ- words; in the case of -π- irregular ictus had to be specially motived, or it could hardly have appeared at all. The occurrence of ὅππ|ως in a speech (P 144) might be taken to prove that petrifaction could eventually bring even this kind of ictus-length into thesis, as happened in the -σσ- forms; but unluckily the instance is very unstable (φράζεό νυν πῶς κεν σύ). The ictus ζευγνῦμεν is late, but not irregular—within the line ζευγνύμεν must give one spondee; the phrase is merely a shift from an Odyssean ζεῦξαι ἐκέλευε. We must next look at the spondees which though not necessarily to be taken as ictus-lengths, yet are similar to those given above, because short scansion was possible:

B) Analogous spondees—χειμάρροος N 138, καλλίρροον B 752, M 33, X 147; ἐς κουλεόν A 220; βάρδιστοι Ψ 530, καὶ κάρτεϊ Θ 226 = Λ 9, καὶ καρτερός ν 393. Speeches—ἀπολλήξῃς O 31, -εις τ 166; ἀνωϊστί δ 92, ἀψορρόου Σ 399, ν 65; type ἧς εἵνεκα B 177, E 651, Ξ 89, Ω 106, (501), γ 140, ν 263, ρ 118; δεῦρω Γ 240; καὶ κάρτεϊ ο 143, σ 139, ὃν καρτερόν E 806, καὶ καρτερός ξ 116, ἀλλ' ὑμεῖς καρτεροί ο 534, καὶ κάρτος γ 370. With the last instances compare κάρτ|ος, speeches—I 254, δ 415, ζ 197. These scansions are analogous to false length when it gives an avoidable spondee:

Narrative—προθυμίῃσι B 588, θείῃ Z 507=O 264, μενοινήῃσι O 82 ποίπνυον Ω 475, ἀπέπλειον θ 501 ; analogous—εἰδυῖα P 5, ἀμοιβηδὶς δέ Σ 506, σ 310, δωτῆρες θ 325, πλεῖον θ 475, and perhaps πολλὰ λισσόμενος Φ 368, X 91.

Speeches—ὑπεροπλίῃσι A 205, ὀκνείω δ' E 255, ἀλόντε E 487, πουλύν Θ 472, ὑποδεξίη I 73, πλεῖον A 165, τριηκόσι' Λ 697, μενοινώω N 79, -άᾳ T 164, ἐρητύοντο O 723, ὅππως κε P 144? νῦν δ' ὅττι ψ 115? πνείει P 447, σ 131, ἐπιπνείῃσιν δ 357, ἐγχείῃ ι 10, ἠγάασθε ε 122, ἀμῷεν ι 135, (τετράκυκλοι ι 242; Ω 324 is as bad), ἀτιμίῃσιν ν 142, ἱστίη ξ 159, ρ 156, τ 304, υ 231, ἀκομιστίη τε φ 284, ἀλλύεσκον τ 150, ω 140, ἀλλύουσαν ω 145. The last three examples are irregular in another way. It seems that apocope giving an avoidable spondee is a very dubious scansion:

Narrative—παρειπών Z 62, H 121, πὰρ νηῶν Ξ 28, κὰδ δ' ἷζον Φ 520, Ψ 28, ἂμ πέτρῃσι ε 156, (κὰδ δ' ἐκ πασσαλόφιν θ 67, 105), κατὰ κρᾶτα θ 92, ἀμβαίνειν ο 548; compare the doubly irregular πρὸς νηῶν O 670.

Speeches—ὑμῖν πὰρ προτέρ. Θ 188, παρειπών Λ 793, O 404, πὰρ νηῶν Θ 533, N 744, Ξ 46, πὰρ νήεσσι X 386, Ω 408, ξ 260, πὰρ νηΐ τε ι 194, κ 444, καλλείπειν K 238, καλλείψειν Ξ 89, καλλείψω ν 208, ἂμ πύργους Σ 278, καδδῦσαι T 25, ὑββάλλειν T 80, ἀλλέξαι Φ 321, ἀμμίξας Ω 529, κατθάψαι Ω 611, κὰδ δώματα δ 72, ἀμβαίνειν ι 178, 562, λ 637, μ 145, ἀμβαίη μ 77, ἀμβαίνωμεν ο 219, κακκῆαι λ 74, πὰρ κείνοισιν λ 175, ἀππέμψει ο 83, καννεύσας ο 464, παρφάσθαι π 287, τ 6, κατθέσθην χ 141 ; cf. τὸν κάμμορον β 351, and πρὸς οἶκον I 147, πρὸς Θύμβρης δ' K 430, πρὸς πάντων Π 85, πρὸς πέτρῃσι ι 284, πρὸς πατρός ν 324, προσφάσθαι ψ 106, and the tendency to ἐν (notably ἐγκοσμεῖτε ο 218.

Since the present subjunctive in similes is not grammatically necessary, the spondees so given are interesting: ἅπτηται Θ 339, ἔρχηται K 185, ἐρίζητον M 423, ἕλκητον ν 32. The finest of these is K 185, combined with δι' ὄρεσφι. In most points K is a handy euchrestic of what is meant by "Odyssean". The subjunctive in -ῃσι is regular in εὕδῃσι E 524, and εὕρῃσι M 302; the use is analogous to displacements like εὕρ|ῃ: but in ἕλκῃσιν Ψ 518, the scansion is irregular, involving -ν length (cf. γ 422). Bacchiacs like ἅπῃσι are fairly common in the speeches, and at last make their way into the 5th trochaic—Θ 405=419, O 109, (T 223, γ 476), δ 357, (591), λ 224, ρ 385, τ 490, φ 229, χ 373; giving antispasts the ending is found in K 511, Ω 651, ε 356,

κ 288, τ 519. Since -ηθεν from -η- stems may represent -ηφιν, the spondaic examples should be compared—narrative, B 838, Θ 304, 397, 438, M 96, η 9, ν 124, speeches, Γ 276, 320, H 202, I 44, Ω 308, κ 216. Similarly -εσκ|ε -ον of the iterative is not found in the narrative of the Iliad*; elsewhere it appears in (narr.) Ω 23, 455, γ 409, ε 154, ρ 294, 331 (cf. ν 3). speeches E 790, Σ 259, X 503, Ω 752, β 104, τ 149, ω 139, η 260, θ 225, ι 184, μ 380, ν 350, τ 574, χ 315. There are several other avoidable spondees which are peculiar to B² etc. (and Od.), e. g. ἱμερτόν B 751, ἐξίκετο Θ 439, Ω 481, ἄξοντο Θ 545, ἱμᾶσι K 475, Ψ 363, φ 46, ἧος (=ἵνα) δ 800, ζ 80, φυλακτῆρας I 80, Ω 444, cf. διοπτῆρα (=ἐπίσκοπον) K 562, from ὀπτῆρας (ξ 261; ἐπισκόπους cannot be used). Noticeable, too, though not so clear as this, is the distribution of certain other spondaic words which have non-spondaic equivalents. Thus μῦθος is most natural in the form μύθοισι replacing ἐπέεσσι; the exceptional spondees are the formula τοῖσι δὲ μύθων ἦρχε and Λ 643, a 361, δ 676, π 398; speeches—B 796, Γ 212, Θ 524, I 443, 627, K 61, 288, Λ 781, M 232, T 84, (Ψ 478), a 273, 358, 373, γ 124, 140, δ 214, (239), 597, 744, 774, λ 368, 442? 561, ν 295, ρ 395, φ 71, 291. Take again the avoidable spondees given by ποιῶ—narrative A 608, Z 316, H 222, Θ 2, 386, K 262, M 5, 30, 432, 470, O 363, Π 171, Σ 371, 490, 573, 587, Υ 12, Ψ 164, Ω 449, 452, δ 796, ε 254, 259, 452, ζ 10, θ 373, π 456, ρ 207, τ 57. The most interesting example here is Ω 449, 452; if the reader cares to substitute τεῦξαν βασιλῆι, he will have before him one of the rarest scansions in Homer—T 359 (really a speech-phrase), δ 228; N 265, δ 569, η 313, ω 309. In the speeches, spondees are found in P 646, Ω 537, 611, 666, and in the Odyssey 20 examples, of which the most notable are ι 524, π 127, φ 71. Other spondaic words are τελεύτησε narrative Ξ 280, β 378, θ 510, speeches Θ 9, N 100, 375, O 74, and 16 occurrences in the Odyssey, ἔκτοσθε narr. H 440, K 263, 334, η 112, χ 385, ψ 148, speeches H 341, I 552, ε 411, ν 101, ἔντοσθε narr. Δ 454, K 262, 334, M 296, 416, 455, and 8 examples in the Od., speeches Z 364, X 237, and 8 examples in the Od., τοιοῦτος narr. only in the formula ὣς οἱ μὲν τοιαῦτα, etc., speeches B 372, Γ 46, H 242, Π 847, P 643, Φ 428, Ψ 494, 644, and 19 examples in the Odyssey; similarly there is in the Odyssey an increasing freedom in the use of spondaic οὗτος, and τοιόσδε, ἐνταῦθα, etc.—two very fine scansions are λ 548, χ 254.

This general tendency is so marked in later work, that it is difficult to choose illustrations out of the huge number offered —words like αἰνίζομαι, αἰτίζω, αἰών, βουλεύω, (especially βουλὰς βουλεύειν, and cf. ι 268), ἐσθής, ἧμος, ἰδρύω, κεχρημένος, κούρη (e. g. κούρη Βρισῆος), μεταλλῆσαι, τόλμησε (special sense), etc.; forms like ἀλλοῖος, ἀρχεύω, ἀφνει|ός, ἤντησε, and perhaps οὐδέν (δ 350, ρ 141); redundancies like 'Αργείων Δαναῶν (θ 578), θνητοῖσι βροτοῖσι, θνητὸς βροτός, ξείνους ἀνθρώπους (cf. ξ 316-7), etc., and conversely γλαυκῶπι for 'Αθήνῃ (ν 389) and αἰδοίοισι for ἱκέτῃσι, ο 373 (cf. ἠοίην δ 447); uses such as spondaic infinitive for imperative (e. g. παρφάσθαι); spondaic periphrases like βίη Τεύκροιο, Λεοντῆος μένος, ἱερὴ ἴς Τ. or 'Αχιλλῆος κεφαλῇ Π.; contraction or synizesis giving spondees (e. g. |κράτων, ὠτειλ|ήν, |Βορέης, |οἰῶν): spondaic displacements such as ||ἑταῖροι, ἵκω -ει, ἱκάνω, κιχάνω—in narrative some displacements are peculiar to B² etc. (and Od.) e. g. πὰρ δὲ ζωστήρ or πρῶτ' ἐμβασίλευε. The rarity of the last scansion shows that ὕδατος πλῆτ' (suggested for Φ 300) is worse than the πλῆθ' ὕδατος of the text; really, dactylic ὕδατος is just as legitimate as terminal ὕδωρ.

This difference between the scansion of the Iliad* and that of the Odyssey comes out in a great variety of details, but as has been said, nowhere better than in the treatment of words like ἀλλ|ος, καλ|ός, etc. The divergence here is glaringly obvious, and must eventually be admitted by all who are interested in Homer. As soon as the fact is admitted, the interpretation will be disputed. The contention will be that in themselves differences of metre do not prove any difference of authorship. No doubt this is quite true; the thing is seen in Shakespeare. But in Homer the differences of scansion go together with differences of language, ranging from pronunciation (e. g. σμ-, σν-, ϝ, and contraction) to matters of syntax —the finest instance of this is the degradation of the generalizing idiom, leading at last to consecutive ὥς τε, οἷός τε, etc., which are peculiar to B² etc. and the Odyssey. That any metrical evidence in Homer is at present a little disparaged, is due to two causes—in the first place, no one yet can quite scan a hexameter (even the avoidance of the 4th trochaic is still unexplained); and secondly the stratification of the Iliad by means of the metre gives a most disappointingly natural result; there is no μῆνις, but there are four self-subsistent ἀρι-

στεῖαι (in E [1], Λ, Π, and P), and then comes work of date varying from the battle piece in Υ right down to the Catalogue. Since the acceptance of this result would involve facing the highly intricate problem of the speeches, and that of the artificial production of an Iliad, it is no wonder that people are almost driven to the deplorable "Homer" of the unitarians.[1]

<div align="right">J. A. J. Drewitt.</div>

Wadham College, Oxford.

[1] Owing to postal difficulties Mr. Drewitt's article has not had the advantage of his revision.—B. L. G.

III.—THE TROJAN WAR AGAIN.

To those who accept the Iliad, substantially as we have it, for the work of a single poet the duration of the war which forms its background will not seem a matter of merely incidental interest. For if we may assume that Homer means to tell the story of what happened a few weeks after the Achaians landed, as Professor van Leeuwen maintains,[1] a good many things which on the traditional view require explanation and apology immediately become reasonable and convincing. Professor Scott has written an interesting paper setting forth his reasons for declining to make this assumption, but that he feels, as indeed everybody must, that it would make the Iliad more human and intelligible is shown by the following words from his closing paragraph:

"Many events, such as the muster of the troops, the report of Iris-Polites as to the multitude of the Greeks, the duel between Paris and Menelaus, the Teichoskopia, and the advice of Nestor in regard to military tactics, do not belong strictly to the tenth year of a siege, but the poet must give us some impression of the appearance of the army, of the tactics to be employed, of the regal bearing of Agamemnon, of the beauty of Helen and her mental attitude regarding her present and former husband, and also the contrast between Paris and Menelaus, but since he did not describe the earlier years of the war he must insert them in the only part he did describe".[2]

This is tantamount to an admission that the Iliad would have been a more perfect poem—because less artificial in its motivation—had the poet not been hampered by a tradition which forced him to lay his scene in the last year of a long war. But before we grant the existence in Homer's day of

[1] Mnemosyne 38 (1910), pp. 354 sqq. = Commentationes Homericae, Leyden, 1911, pp. 1 sqq. This article is a revision (with considerable modifications) of one which appeared in Mnemosyne 34 (1906), pp. 193 sqq.

[2] On the Assumed Duration of the War in the Iliad. Class. Phil. 8 (1913), pp. 445 sqq.

this long-war tradition, and make such excuses as we can for the inappropriateness of these episodes in such a setting, we should consider if there be not other things in the Iliad which also point to a short war, things which cannot be explained as forced upon the poet by an embarrassing tradition.

Take Achilles. If Homer's sources obliged him to think of Achilles as having been in camp before Troy for nine years at the moment when the action of the Iliad begins, it is clear that they cannot also have compelled him to make Achilles out a youth of eighteen or twenty; yet that is exactly what he has done. Homer nowhere tells us the age of his hero, but not to see that he is portraying a noble and impulsive *youth* would mean the failure to appreciate a character-delineation which is perhaps the greatest achievement of the Greek epic.[1]

The evidence afforded by the poet's treatment of Helen is, of course, less extensive, and maybe less definite, but there are not wanting signs that Homer thinks of her as very young. Her hot resentment against her abductor, her keen desire that Menelaus may be victorious, her almost overwhelming sense of shame in the false position she occupies in Troy—these emotions are too poignant by half to be natural in a woman who has lived ten (or must we say even twenty?) years as the wife of Paris and a princess among the Trojans.

A very definite indication of the brevity of the war is seen, however, in certain words spoken by Hermes in Ω. The god has come to meet Priam and guide him safely to the hut of Achilles. "He went", says the poet (vss. 347 sqq.), "in the likeness of a young prince, with the first down upon his cheeks", and when Priam asked who he was and of what parents, Hermes answered "I am one of the Myrmidons, and my father is Polyktor". This shows clearly that to the writer of these verses the war had not been going on nine years, for Hermes tells us that the son of Polyktor had left his home to follow Achilles to the Troad, and at the close of the action of the Iliad he is still so young that the down is just appearing on his cheeks. The indirect and off-hand way in which this

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[1] This portrait of the youthful Achilles owes its vividness, in large measure, to a considerable number of little touches. There is a résumé in van Leeuwen's Comm. Hom., pp. 24–26= Mnem. Tom. Cit. pp. 375–377, with citation of the passages.

valuable hint is conveyed makes it the more convincing. The poet quite evidently does not expect us to be surprised that a soldier who has been with Achilles from the beginning should be scarcely more than a boy as the war draws to a conclusion. Professor Scott thinks that Homer had no intention of giving Hermes the form and attributes of the son of Polyktor, but was merely describing the god as tradition represented him. " When once an actor is given or supposed to have a definite form or age ", says Mr. Scott, " he maintains each unchanged throughout ", and he rather sternly taxes Professor van Leeuwen with " an utter failure to grasp " this principle, which he terms " one of the fundamental laws of Homeric poetry ". But as we read on, we learn that this " law " is derived from a consideration of certain passages in the Odyssey, and to those (of whom Professor van Leeuwen is one) who believe that the Odyssey is the work of a later poet than the author of the Iliad there will seem little justification for employing it to determine the interpretation of a passage in the older poem. Though for that matter the " law " in question does not seem to be applicable, in either poem, to the appearances of gods to men.[1] Iris in B 795 assumes the figure of Polites, and in Γ 121 that of Laodike. Athene comes to Telemachos in the guise of Mentes in α 105, and to Nausikaa in that of the daughter of Dymas in ζ 22. Obviously a god can assume any form he chooses, for the nonce, and I see no reason to doubt that the youthful look of Hermes here is merely the look of the young Myrmidon. Professor Scott calls attention to the fact that " Hermes appeared to Priam in exactly the same form as he appeared to Odysseus in κ 279". But it would be a little hazardous to conclude from these two epiphanies (especially since the verse is obviously borrowed in one of these passages from the other one) that extreme youthfulness was an Homeric attribute of Hermes. Dr. Leaf remarks in his note on Ω 348 that " it is strange that the description should suit only the youthful Hermes of the

[1] Professor Scott also brings his law to bear upon Mr. van Leeuwen's arguments from the youthfulness of Helen. Here his citations showing the Odyssean conception of Helen as in the prime of her youth would certainly afford some countenance to his interpretation of Homer's treatment of her in the Iliad,—if one accepted the common authorship.

great age of Greek art; for in works of the archaic period the god is always represented as bearded ". Strange perhaps in the Odyssey passage, where no disguise seems intended, but not in Ω, where the god is pretending to be a certain man and naturally counterfeits that man's appearance of youth.

Again, that the war is still young is the most natural inference to be drawn from the mention of so many Trojan allies as newly arrived in the Troad. This evidence Professor Scott seeks to explain away by the assumption that Troy did not summon her allies till the war had been going on nine years,[1] i. e. until the Achaians, seeing that Troy could not be taken by storm, determined to cut off her supplies and executed the raid which Dr. Leaf calls the Great Foray. This coup compelled the Trojans to fight, and accordingly they summoned all their allies, and relinquishing their former policy of passive resistance, took the field in force and attacked the Achaians, who were now, in their turn, put upon the defensive and hastily constructed the trench and wall. The allies were not called in earlier because (1) numbers were not necessary to defend the strong fortifications, and (2) they would have consumed food needed by the regular garrison, and so have shortened the period of successful resistance. The real cause for the change in the Trojan plan of campaign, namely the success of their enemies in cutting off their supplies, was ignored by Homer, who substituted for it the motif of the Wrath, which he introduced at the moment when economic causes had obliged the Trojans to assume the offensive.

This ingenious attempt to explain the tardy arrival of many of the allies as a reflection of the actual course of events in a historical ten years' war sets up a hypothesis involving serious difficulties. It seems most unlikely that the invaders would devote nine years to ineffectual attacks upon the city, before hitting upon the obviously sensible plan of maiming Troy's dependencies,[2] and striking at her sources of supply. Again, it is hardly probable that Priam would have endured the risks and hardships of a war at his very gates, for nine years,

[1] Professor Scott says "several years", but as he goes on to refer to the Great Foray as occasioning the change of plan, the preliminary stage of the war will have lasted nine years.

[2] Cf. Leaf's Troy: a Study in Homeric Geography, p. 316 sqq.

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without calling upon his allies for help. There never was a
siege, in the proper sense of the word, and the Achaians never
attempted an actual investment of the city. The Foray was
doubtless intended to cripple the resistance of the Trojans by
the seizure of treasure, cattle, and other means of prolonging
the struggle; but no systematic attempt seems to have been
made to prevent the allies from coming and going freely,
either before or after the Foray, nor can Priam have had any
occasion to fear that he might be unable to import supplies
for them. Moreover Troy subsisted chiefly, if we accept Dr.
Leaf's conclusions as to her *raison d'être*, which I understand
Professor Scott to do, upon the tolls she was able to exact
from the concourse of traders who gathered annually in the
Trojan Plain for the great Fair over which King Priam pre-
sided. It seems therefore more than doubtful that she would
have tamely suffered the interruption of this fair for nine sum-
mers without mustering all her resources in an effort to dis-
lodge the interlopers. Finally, it is unreasonable to suppose that
Troy's influence with these allies of hers—an influence based
on commercial considerations—could have been so closely
maintained during nine years of commercial sterility that at
the close of this period she could still command their services
in her defence.[1] It appears then much more likely, on *a
priori* grounds that Professor van Leeuwen's long list of
allies who have recently arrived is best accounted for by the
explanation which he himself offers, namely that Priam issued
his appeal for help on first learning of the prospect of inva-
sion, and that while some of the forces arrived before the
enemy, others, like the remote Paionians, only turned up when
the summer was already half over.[2]

[1] This would apply especially to the Lykians, whose carrying trade
between the South and the Troad was the life-blood of their national
prosperity. See Dr. Leaf's Troy, pp. 320–322.

[2] Dr. Leaf says (Troy, pp. 318 sqq.) : "Troy, though it cannot be
taken, is being slowly bled to death by the exhaustion of its economic
resources. Though the entry of troops and food cannot be hindered,
it is at least possible to make it a place to which no trader will come,
and so to exhaust the accumulated wealth which has sprung from its
favoured position in the past. This is the picture which is put before
us throughout, most clearly in the words of Hector (xviii. 287–92) :
'Have ye not had your fill already of being pent behind the towers?

Another indication that Homer's sources told of a short war is to be found in the fact that the dozen mostly casual references to captives and booty taken from captured cities of the Troad all point to a single expedition, when Achilles, shortly before the action of the poem commences, raided Thebe, Lyrnessos, Pedasos, and probably Lesbos, and Tenedos. In a long war there would be many such raids, and the poem which has preserved twelve scattered references to this one would hardly have escaped some allusions to others.[1]

It appears then, to sum up the case for a short war, that not

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Of old time all mortal men would tell of the city of Priam for the much gold thereof, but now are its treasures perished out of its dwellings, and much goods are sold away to Phrygia and pleasant Maionia, since mighty Zeus dealt evilly with us'. Hector's argument is, 'you have tried the policy of sitting still within your walls, where you are safe; and the result is that you are being worn down by loss of wealth. The only safety that we can find now is in the offensive'.

"Achilles too speaks of the impoverishment of the rich city (ix. 401) : 'For not of like worth with life hold I even all the wealth that men say was possessed of the well-peopled city of Ilios in the days of peace gone by, before the sons of the Achaians came'. Of the burden of subsidies to the allies we hear in xvii. 225 : 'For this end am I (Hector) wearying my people by taking gifts from them and nursing thereby the courage of each one of you'. The picture vividly describes the plight of a city whose wealth is founded on commerce only; which depends upon an army sufficient for police purposes in times of peace, but inadequate to meet a foreign invader. Its only resource in the emergency is to hire mercenaries from outside; and where should they be sought but among the nations who had a vested interest in the maintenance of existing trade against an aggressive and growing people who sought to control commerce to their own advantage"?

It will be noticed that Dr. Leaf conceives of Troy's loss of wealth as chiefly due to the expense of maintaining a mercenary army. Professor Scott, who also cites these passages, does not expressly attribute the depletion of Troy's resources to this cause, perhaps feeling that since his hypothesis involves only one season's employment of mercenaries he must account for it rather by the need of buying supplies for the regular garrison for a period of several years. To my mind, the financial embarrassment which is here hinted at might conceivably have come very swiftly to a community such as Troy seems to have been, when it found itself obliged to subsidize a large army of foreign troops. The verses cited contain therefore no answer, that I can find, to the question whether the war was a matter of years or of months.

[1] I have presented the evidence for this argument in A. J. P. XXXV (1914), pp. 294 sqq.

only are there many important passages in the Iliad which are
more natural and intelligible upon this view of the matter, but
that there are also certain unequivocal proofs that this was
the form of the legend known to Homer (I refer particularly
to the many passages implying the extreme youth of Achilles,
and to the Hermes passage). The numerous references to
newly-arrived allies are best explained in this way. And
finally there is the argument ex silentio to which I have just
called attention.

Why then has it been universally supposed that the ten-year
tradition was followed by Homer?

First, because there are four passages in our Iliad where
this is as good as stated; second, because there are several
other places where it seems to be implied; third, because the
older, short-war, tradition had been supplanted by the later,
ten-year, form of the story, as early as the date of the compo-
sition of the Odyssey, since the chronology of the later poem
is evidently based on the assumption that Odysseus had been
ten years in the Troad.

From the time of the Odyssey, the short-war tradition is no
more heard of, and there would be no evidence that it had
ever existed, were it not for the traces of it to which Pro-
fessor van Leeuwen has directed our attention in the Iliad
itself.

Two questions are at once suggested. How came the leg-
end to be so radically modified during the period which lay
between the composition of the Iliad and that of the Odyssey?
And how came the older poem to admit interpolations incon-
sistent with its own form of the story?

Professor van Leeuwen has not failed to realize how neces-
sary it is for him to answer these questions in a satisfactory
manner, if he hopes to find acceptance for his theory. His
explanation may be summed up as follows:

In the old stories which celebrated the Trojan War were
preserved vague traditions of various expeditions made by
Greeks to the coast of Asia Minor. Some of these traditions
had formed a part of the legend, from the beginning; others
were subsequently incorporated with it. The cycle of stories
which the earliest Achaian settlers had carried with them to
Asia Minor assimilated, from time to time, the names of a

good many ancient heroes who had come over to those coasts, at one time or another, from various parts of Greece. With the lapse of years a considerable number of these stories came to be connected by fictitious genealogical ties, and along with the increasing compass of the events thus associated there came a gradual lengthening of the period within which they might reasonably be presumed to have occurred—the poets being solicitous lest the heroes of their songs appear contemporary with their own fathers, or even their grandfathers.[1]

The Greeks did not readily distinguish between earlier and later, in dealing with their epic poetry, and as they read the Iliad they inevitably confused with its ideas notions which had grown familiar to them from the works of later poets. Odysseus had been absent from his home twenty years: ergo the Trojan War had lasted ten years. Menelaos had required several years to get his fleet together: thus Helen was in her twentieth year at Troy when she mourned over Hektor. Troy had been taken by Neoptolemos, begotten by Achilles, before the war, in Skyros: hence the preparations for the war and its prosecution must have lasted long enough for Neoptolemos to grow up. It was at the marriage of Peleus and Thetis that Eris set the goddesses to quarreling, and their rivalry shortly led to the rape of Helen, when Achilles was not yet born: therefore Troy was captured by Neoptolemos about forty years after Paris won Helen for his wife. In this way the years were multiplied, to correspond with the increasing mass of stories which had come to be connected with one another, and it is lucky that the late poet who put these words in the mouth of Helen,

ἤδη γὰρ νῦν μοι τόδ' ἐεικοστὸν ἔτος ἐστίν,
ἐξ οὗ κεῖθεν ἔβην καὶ ἐμῆς ἀπελήλυθα πάτρης· (Ω 765 sq.)

should have made her only forty years old, and not sixty, for the latter age would be suggested, not indeed by common sense, but by a scrupulous reckoning of the years which had elapsed since the marriage of Peleus.[2]

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[1] Mnemos. Tom. cit., p. 384 = Commentationes Homericae, p. 33 sq.

[2] Mnemosyne, Tom. cit., p. 374 = Commentationes Homericae, p. 23.

It would almost seem that Professor Scott must have overlooked these passages when writing his criticism of Professor van Leeuwen's theory. Otherwise he could hardly have penned the following sen-

Let us now consider how far this interweaving of legends, with its attendant readjustment of chronology, will have affected the actual text of the Iliad. Besides the passage just quoted from Helen's lament, there are three others which must be regarded as interpolated, or at least corrupted, after the short-war legend had been forgotten. These are (1) Agamemnon's words in B 134 sq.,

> ἐννέα δὴ βεβάασι Διὸς μεγάλου ἐνιαυτοί,
> καὶ δὴ δοῦρα σέσηπε νεῶν καὶ σπάρτα λέλυνται ·

(2) the passage a little further on, where Odysseus is describing the omen at Aulis, especially 295 sq., 313, and 326 sqq. :

> ἡμῖν δ' εἴνατός ἐστι περιτροπέων ἐνιαυτὸς
> ἐνθάδε μιμνόντεσσι ·
>
> . . . .
> ὀκτώ, ἀτὰρ μήτηρ ἐνάτη ἦν, ἢ τέκε τέκνα
>
> . . . .
> ὡς οὗτος κατὰ τέκν' ἔφαγε στρουθοῖο καὶ αὐτήν,
> ὀκτώ, ἀτὰρ μήτηρ ἐνάτη ἦν, ἢ τέκε τέκνα,
> ὡς ἡμεῖς τοσσαῦτ' ἔτεα πτολεμίξομεν αὖθι,
> τῶι δεκάτωι δὲ πόλιν αἱρήσομεν εὐρυάγυιαν.

and (3) M 15 :

> πέρθετο δὲ Πριάμοιο πόλις δεκάτωι ἐνιαυτῶι.

On these four passages the case against Professor van Leeuwen must chiefly rest. If one is ready to extenuate any inconsistencies in the Iliad sooner than allow the possibility that any part of the traditional text may be interpolated, one need not stop for further proofs, for if these lines are by

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tences : "There are no better attested verses in Homer than the references to the long war in the speeches of Agamemnon and Odysseus. They created no impression in ancient times that they contradicted the rest of the poem. Such a theory presupposes that the Iliad was in the keeping of men who had complete control of the text, so that they were able to change it at will. Unless there was such absolute control, how was it possible to effect the adoption of additions or mutilations which were out of keeping with the whole? If the Iliad teaches that the events all belong to a single summer, how, in the face of this tradition did the other tradition become so powerful? When once the Iliad became generally known, such interpolations would have been impossible : hence the two traditions must have been practical contemporaries. Why was the tradition of the one summer's war so strong that it could form the background of the Iliad, and then immediately so weak that it must yield to the tradition of the ten years' war? How,

Homer it is clear that the short war cannot have been the basis of his poem. But if one is prepared to admit that the retention of these verses may perhaps involve more improbable assumptions than their rejection, the next question will be what further indications the poem may contain that its author thought of a war of ten years. Such indications are in fact neither numerous nor positive. I give first those which Professor van Leeuwen has listed in the critical note to B 134 sq., in his new edition of the Iliad.

Γ 156 sq. The Trojan elders say, when Helen is seen approaching :

οὐ νέμεσις Τρῶας καὶ εὐκνήμιδας 'Αχαιοὺς
τοιῆιδ' ἀμφὶ γυναικὶ πολὺν χρόνον ἄλγεα πάσχειν·

I do not feel at all sure that we have here, in the phrase πολὺν χρόνον, a reference to years of fighting. To the weary elders, pent within their walls, five or six weeks [1] may well have seemed "a long time".

Ζ 123 sqq. Diomedes to Glaukos :

τίς δὲ σύ ἐσσι, φέριστε, καταθνητῶν ἀνθρώπων;
οὐ μὲν γάρ ποτ' ὄπωπα μάχηι ἔνι κυδιανείρηι
τὸ πρίν·

This seems to imply that there have already been battles between the Achaians and the Trojans.

Ζ 419 sq. Andromache speaks of the elms that grow about

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in the face of the Iliad, did the later tradition arise? The very conception of changing the plan or details of a poem involves the existence of a group of men having the poem in absolute control" (p. 446 sq.).

It is obviously not the Leyden scholar but a man of straw who is here assailed, for Professor van Leeuwen assumes that the interpolations which point to a long war were the product of an age which knew not the tradition of a short war. The lines in question were not introduced into the poem because anybody wanted to "change the plan or details" of the Iliad. Indeed their introduction was only possible precisely because they did *not* involve any change in what people then assumed had always been believed about the Trojan War.

[1] We are now, according to van Leeuwen's computation, in the twenty-third day of the action of the Iliad, and he estimates about fifteen days from the landing to the opening scene in A.

the grave of her father in a way which suggests that Eetion has been dead some years:

περὶ δὲ πτελέας ἐφύτευσαν
νύμφαι ὀρεστιάδες, κοῦραι Διὸς αἰγιόχοιο.

But this passage is not a whit more inconsistent with the short-war hypothesis than with the traditional chronology of the poem. In either case Achilles slew Eetion in the Great Foray, and the Great Foray, with the capture of Chryseis, led immediately to the opening events of the poem.

Σ 434 sq. An allusion to Peleus as spent with age:

ὁ μὲν δὴ γήραϊ λυγρῶι
κεῖται ἐνὶ μεγάροις ἀρημένος,

Professor van Leeuwen thinks that Homer regarded Peleus as a man in middle age. The connecting of the Trojan War story with the story about his marriage made an old man of him.

T 326–337, and Ω 466 sq. References to the son of Achilles:

ἠὲ τὸν ὃς Σκύρωι μοι ἔνι τρέφεται φίλος υἱός,
εἴ που ἔτι ζώει γε Νεοπτόλεμος θεοειδής.
πρὶν μὲν γάρ μοι θυμὸς ἐνὶ στήθεσσιν ἐόλπει
οἶον ἐμὲ φθίσεσθαι ἀπ' Ἄργεος ἱπποβότοιο
αὐτοῦ ἐνὶ Τροίηι, σὲ δέ τε Φθίηνδε νέεσθαι,
ὡς ἄν μοι τὸν παῖδα θοῆι σὺν νηὶ μελαίνηι
Σκυρόθεν ἐξαγάγοις καὶ οἱ δείξειας ἕκαστα,
κτῆσιν ἐμὴν δμῶάς τε καὶ ὑψερεφὲς μέγα δῶμα.
ἤδη γὰρ Πηλῆά γ' ὀίομαι ἢ κατὰ πάμπαν
τεθνάμεν, ἤ που τυτθὸν ἔτι ζώοντ' ἀκάχησθαι
γήραϊ τε στυγερῶι καὶ ἐμὴν ποτιδέγμενον αἰεὶ
λυγρὴν ἀγγελίην, ὅτ' ἀποφθιμένοιο πύθηται.

τύνη δ' εἰσελθὼν λαβὲ γούνατα Πηλείωνος,
καί μιν ὑπὲρ πατρὸς καὶ μητέρος ἠυκόμοιο
λίσσεο καὶ τέκεος,

Professor Scott does not cite these passages in his own review of the evidence, though if genuine they are undoubtedly a strong argument for the traditional view. But they have long been suspected of being interpolations, "due to a desire to bring into the Iliad so prominent a hero of the later cycle" (Leaf, on T 327). It may be noted, in passing, that the author of the lines in T conceives of Neoptolemos as a child "too young to travel alone, much less to fight" (Leaf);—a notion hardly more compatible with the long war than with

the short one, for Homer shows that he thought of the war as
destined to be ended soon after the death of Achilles,[1] and
one who is now spoken of as a child will not be ready in a few
weeks to replace the Achaian champion.[2]   There is also in the
second of these two passages a rather striking indication of
interpolation, quoted by Dr. Leaf from Düntzer, to wit:
" Priam does not follow the god's advice, as he makes no
mention of either mother or son ".

These are all the places in our text which seem to Professor
van Leeuwen to be inconsistent with his theory.   But Pro-
fessor Scott cites a number of others which he regards as
pointing to a long war, more or less clearly, and to these we
must now turn our attention.   I will frankly confess that I
can discover no confirmation of the traditional view in any of
them.   It must be remembered that the new theory assumes
that the Achaians have already been some two or three weeks
in the Troad, when the Iliad begins.[3]   They have captured
Thebe and other towns, and have taken much booty and many
prisoners.   Protesilaos was killed when the invaders landed.
There has been some guerrilla fighting in the Trojan Plain.
And, finally, the pestilence has carried off large numbers.
These facts are sufficient to explain the following passages:

B 115.   Agamemnon says:

> ἐπεὶ πολὺν ὤλεσα λαόν.

B 161 sq.

> Ἀργείην Ἑλένην, ἧς εἵνεκα πολλοὶ Ἀχαιῶν
> ἐν Τροίηι ἀπόλοντο, φίλης ἀπὸ πατρίδος αἴης.

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[1] Cf. van Leeuwen, Mnem. Tom. cit., p. 371 = Comm. Hom., p. 19.

[2] Dr. Leaf remarks that "such anachronisms are a small matter to a
poet seeking for pathos", but an interpolator whose eye was on the
detail he was seeking to embellish would be much more likely to disre-
gard its relations to the whole scheme than the author of the poem
would have been.

[3] I do not know what Professor Scott means by speaking of "the
theory of van Leeuwen that the first intimation that came to the Tro-
jans of the landing of the Greeks is found in the speech of Iris who
assumed the form of Polites (B 796 ff.)".   Was the battle which
attended the landing of the Achaians not yet known to the Trojans on
this theory?   Had they not yet had any hint of the Foray, in which
their allies had suffered so heavily?   Had Aineias not mentioned his
little adventure on the crest of Ida?   Did the Trojans know nothing
of the pestilence which had been destroying their enemies a few miles
away?

B 272 sq.   The soldiers say to one another:

> ὢ πόποι, ἦ δὴ μυρί' 'Οδυσσεὺς ἐσθλὰ ἔοργε
> βουλάς τ' ἐξάρχων ἀγαθὰς πόλεμόν τε κορύσσων.

Γ 99 sq.   Menelaos says to the Achaians:

> ἐπεὶ κακὰ πολλὰ πέπασθε
> εἵνεκ' ἐμῆς ἔριδος καὶ 'Αλεξάνδρου ἕνεκ' ἀρχῆς.

Γ 125 sqq.   Iris finds Helen at her web:

> τὴν δ' εὗρ' ἐν μεγάρωι· ἡ δὲ μέγαν ἱστὸν ὕφαινε,
> δίπλακα πορφυρέην, πολέας δ' ἐνέπασσεν ἀέθλους
> Τρώων θ' ἱπποδάμων καὶ 'Αχαιῶν χαλκοχιτώνων,
> οὓς ἔθεν εἵνεκ' ἔπασχον ὑπ' "Αρηος παλαμάων.

Γ 130 sqq.   Iris calls Helen to the wall:

> δεῦρ' ἴθι, νύμφα φίλη, ἵνα θέσκελα ἔργα ἴδηαι
> Τρώων θ' ἱπποδάμων καὶ 'Αχαιῶν χαλκοχιτώνων·
> οἳ πρὶν ἐπ' ἀλλήλοισι φέρον πολύδακρυν "Αρηα
> ἐν πεδίωι, ὀλοοῖο λιλαιόμενοι πολέμοιο,
> οἱ δὴ νῦν ἕαται σιγῆι. πόλεμος δὲ πέπαυται.[1]

E 788 sqq.   Here (as Stentor) says:

> ὄφρα μὲν ἐς πόλεμον πωλέσκετο δῖος 'Αχιλλεύς,
> οὐδέ ποτε Τρῶες πρὸ πυλάων Δαρδανιάων
> οἴχνεσκον· κείνου γὰρ ἐδείδισαν ὄβριμον ἔγχος·

H 113 sq.   Agamemnon dissuades Menelaos from facing Hector by saying:

> καὶ δ' 'Αχιλεὺς τούτωι γε μάχηι ἔνι κυδιανείρηι
> ἔρριγ' ἀντιβολῆσαι, ὅ περ σέο πολλὸν ἀμείνων.[2]

Besides the above passages, Professor Scott cites also:
I 351 sq.   (Achilles speaks),

> ἀλλ' οὐδ' ὣς δύναται σθένος "Εκτορος ἀνδροφόνοιο
> ἴσχειν·

[1] The context seems to require that πρίν should here be taken in the sense of *modo*, not *antehac*, as Mr. Scott implies by quoting the passage as an argument for the long war.

[2] I include this passage with the rest of those which seem sufficiently accounted for by the fighting, etc., which has already taken place, even on the short-war hypothesis. But it seems altogether improbable that it reflects any traditional incident, at all. In I 352 Achilles boasts that so long as he was in the fighting Hektor never ventured far from the walls, and Dr. Leaf points out that this passage (also cited by Mr. Scott, among the indications of a long war) contradicts H 113, which he pronounces "a rhetorical exaggeration used at the moment for a special purpose".

" The fact that Achilles speaks of Hector", says Mr. Scott, " as the 'manslaying', exactly as he did in A 242, shows that this title or reputation must have been won in battles fought before the quarrel described in the first part of the Iliad". Does this mean that pre-Homeric accounts of the war must have told of earlier battles in which Hektor had 'got his man', and that Homer, finding the epithet there applied to Hektor, employed it in the places cited, in spite of the fact that he had himself left out the story of those earlier battles? If this were Mr. Scott's meaning one might suggest that it would seem a very natural and excusable thing in the poet, finding Hektor anywhere thus described in earlier lays, to adopt the complimentary epithet in his own allusions to him, without being scrupulous to reserve it until Hektor had actually done some man-slaying in the Iliad. But such is apparently not Professor Scott's meaning, for he has written a paper to show that " Hektor as he appears in Homer, is the creation of the poet who conceived the idea of the Iliad" (Class. Phil. 8 (1913) 160 sqq.). His argument must therefore be something like this: Homer imagined his creature Hektor to have been an experienced warrior at the time the Iliad begins; hence he must have thought of the Trojan War as having already lasted some years. But even assuming, with Mr. Scott, that the poet must have imagined the details of his Trojan hero's past, it does not follow that he conceived Hektor's man-slaying to have been done in this particular war, so that the epithet cannot be regarded as throwing any light on the problem under discussion.

Z 521 sq.

δαιμόνι', οὐκ ἂν τίς τοι ἀνήρ, ὃς ἐναίσιμος εἴη,
ἔργον ἀτιμήσειε μάχης, ἐπεὶ ἄλκιμός ἐσσι·

Hektor thus addresses Paris, and Professor Scott comments: " If this is the first day's fighting of the war, Paris has had little opportunity to show his bravery". But in the paper just quoted he maintains that " Paris was the traditional leader and champion of the Trojans... Paris is no coward in Homer and no weakling, since his heroic proportions show through, despite the efforts of the poet to represent him as mean and timorous". Is not Z 521 perhaps one of the places where Paris's heroic reputation shows through? And might not

the want of sufficient justification in the Iliad for such a compliment, which Mr. Scott now explains as due to Homer's having omitted the earlier stages of the war, be ascribed to Homer's reluctance to accord Paris his traditional deeds of valour?—or, in Mr. Scott's words, to "the efforts of the poet to represent him as mean and timorous"?

B 13 sq.   The Dream says:

> οὐ γαρ ἔτ' ἀμφὶς 'Ολύμπια δώματ' ἔχοντες
> ἀθάνατοι φράζονται ·

I can see here no "vague reference to a long war", as does Mr. Scott. Apollo has already shown his anti-Achaian sympathies, in the pestilence, and Zeus has already shown his friendship for Aineias, whom he has saved from capture in the Great Foray (Υ 92 sq.).   May not the Dream merely mean to imply that Apollo and Zeus are now ready to grant Here and Athena a free hand in assisting the Achaians?

B 344 sq.   Nestor to Agamemnon:

> 'Ατρείδη, σὺ δ' ἔθ', ὡς πρίν, ἔχων ἀστεμφέα βουλὴν
> ἄρχευ' 'Αργείοισι κατὰ κρατερὰς ὑσμίνας.

There is surely no need to see here an implication of previous battles, but only of previous steadfastness of purpose.

N 259 sqq. Idomeneus and Meriones boast of the many Trojan spoils they have taken.   "As neither of these warriors", says Professor Scott, "has thus far more than fleshed his sword in the action of the Iliad, where did they capture all this armor, and in what battles did they perform their mighty exploits"?   Surely there has now been ample time for these heroes to have slain many enemies.   And why might not the poet take this method, as well as another, of letting us know that they have done so?   Does Homer mean us to suppose that there have been no encounters in the battles he has described, save those which he has expressly mentioned?

Lastly, in Ω 257, Priam complains of the loss of three sons, Mestor, Troilos, and Hektor, but Hektor alone has had any share in the action of the poem.   "Hence", says Mr. Scott, "the career of the other two must belong to earlier events". Why not to contemporaneous events which the poet found no room to chronicle?   One would suppose that when Homer composed the Iliad there were current many κλέα ἀνδρῶν which

he worked up for his new poem, and many others which, for one reason or another, he did not see fit to use. That these rejected materials dealt with events prior to the action of the Iliad would be an unwarranted assumption. It is quite possible that the brave deeds of Meriones and Idomeneus, as well as the story of Mestor, and that of Troilos, were related in songs which were known to Homer, and that in the passages cited by Professor Scott these legends "show through",—to borrow his own significant phrase.

It cannot be denied that these lines, and many of the others in which Mr. Scott has seen an argument for the long war, are perfectly in harmony with that tradition. More than that, it may be granted that the reader who takes up the poem in the belief that it deals with the last year of a long war will readily and naturally discover references in these verses to events of earlier years. But if I have succeeded in showing that such an interpretation of them is never necessary, it follows that they cannot be accepted as evidence for the traditional view.

When Professor Scott and his fellow-believers in the common authorship of Iliad and Odyssey succeed in establishing the truth of their contention, Professor van Leeuwen's theory of the war will have to go the way of all flesh and most conjectures. But until that time comes it is to be hoped it may receive the consideration merited by an interpretation which so greatly enhances the beauty and verisimilitude of the Iliad.

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IV.—GRABOVIUS—GRADIVUS, PLAN AND POMERIUM OF IGUVIUM.

The efforts to solve the mystery of the word "Grabovius", an epithet of three gods in the Iguvian tables has thus far been fruitless. The latest summary of the situation, by Samter in the Pauly-Wissowa (V. 14, S. V.) frankly says (p. 1686) : " die Bedeutung und Etymologie des Namens ist ganz unsicher."

The word is Umbrian and appears only in these Iguvian tablets as an epithet of Jupiter, Mars and an unidentified god Vufiume or Vofione. An archaic way of spelling the word in tablet I is Krapuvi. Following the order in Samter's summary we find that it has been variously interpreted as *gra-bos,* a god who feeds cattle (Lassen) ; as *gravis,* a gracious god (Pott) ; as *Krap-* κρατ a strong god; as derived from an original *Grabus* like Fisovius from *Fisus* (Aufrecht-Kirchhoff) ; as equivalent to lat. *gradus,* meaning ' growth ' and, like *gradivus,* meaning the god who produces growth in nature (Bréal) ; and, finally, as the equivalent of "Gradivus" with the meaning of *gradior* (Keller).

Since philological speculation has left us in this *impasse* it may be worth while to consider the problem from the angle of archæology, and in order to do so something needs to be said as to the Iguvian ritual in connection with the topography of the city, because it is in what appears to be the pomerial ritual that the real explanation of Grabovius is found.

The subject of this paper is mainly, therefore, the shape and arrangement of the plan of Iguvium and its pomerium. The Iguvian tables are conceded to represent archaic forms of pre-Roman ceremonial, though preserved only in late copies of the second or even first century B. C. They give the only long formulas of land consecration that we possess, and were hand-books of instruction for the priesthoods of Iguvium. Of the two principal ceremonies, one is for the annual purification of the area of the city and citadel with the renewal of their boundaries; the other is for the annual purification of the

people themselves. They correspond to the Roman *amburbium, ambarvale* and *lustratio populi.*[1]

It is in the text of the first of these ceremonials that the term Grabovius is used. There are two versions of the ritual: a longer version in tablet VI; a shorter and more archaic in tablet I. The ritual in this *amburbium* is as follows: The auspices having been found favorable the priest proceeds first to the city gate called Porta Treblana. Here, *in front* of the gate he sacrifices three oxen to Jupiter *Grabovius* (I a 2; VI a 22) repeating a long prayer on behalf of the *arx* and the city. This prayer he recites three times, once for each victim. He then reënters the gate and at a point *inside* sacrifices three fallow sows to Trebius Iovius, with similar prayers. He goes on, after this, to the second gate, the Porta Tesenaca, where he sacrifices *outside* the gate three oxen to Mars *Grabovius* (I a 11; VI b 1) and *inside* the gate three sucking pigs to Fisus Sancius. Then on the third or Veian gate, where three oxen are offered *outside* to Vofionus *Grabovius* (I a 20; VI b 19) and *inside* the gate three sheep to Tefrus Iovius.

The sacrifices around the border-line of the city having been completed the sacred procession reënters it and proceeds first to the temple or *lucus* of Jupiter where three young bulls are offered to Mars Hodius and, finally, ends the ceremony at the temple or *lucus* of Coredius (Coredier) where three young bulls are offered to Hondus Serfius.[2]

The important feature of the ritual in this connection is that at each of the three gates of the city there is not a single but a double sacrifice. Neither sacrifice is at the gate itself; the first is at a point outside; the second at a point inside the gate. In all three cases of sacrifice *outside* the gate the epithet *Grabovius* is given to the god to whom the sacrifice is offered; in no case is it given to a god to whom an offering is made *inside* the gates. Is this merely a coincidence or has it a meaning?

What are these two sacrificial points outside and inside the

[1] Bréal, Les Tables Eugubines; Buecheler, Umbrica; Buck, A Grammar of Oscan and Umbrian, p. 260 ff. Aufrecht-Kirchhoff, Umbr. Sprachdenkm. etc. II; Conway, The Italic Dialects, I. 399 ff.

[2] The procedure is identical in the two versions of the ritual; there is merely more of detail in tablet VI which contains the text of the prayers that do not appear in tablet I.

gates? What can they be but the outside and inside boundary lines of the pomerium strip on which the walls were built? There has been considerable fluctuation of opinion in regard to the pomerium. It has been denied that it was a consecrated ribbon of land surrounding the *urbs*. It has been said to be merely a single boundary line or ditch. It has been run inside and parallel with the walls; or, again, outside the walls only. It has been imagined as an outside boundary strip not parallel with the line of the walls but as of curved form, like a magic circle. The bulk of scientific opinion has now returned, how- ever, to Livy's definition that it was a strip of land on which the walls were built and which was wider in the section outside the walls and narrower in the band inside the walls, to suit the best policy of defense. I have myself contributed toward defining the width of the pomerial strip outside the walls by pointing out that the arches commonly called triumphal which were built outside the walls of certain Roman cities or on the border lines of the later unfortified colonies and municipalities throughout the Roman world, were placed on the outer boundary of the pomerial strip.[1] It was this outer boundary that was of the greatest importance. Here it was that jurisdic- tion changed from military to civil, as the *urbs* was approached, or *vice-versa;* where the area consecrated by the auspices was entered; where the rule of the city magistrates began. It was at this point that the ceremony of reconsecrating the urban area would naturally begin, as it does in the Iguvian ritual. It would be at this point, just beyond each gate, that the guardian gods of the *urbs* would be asked to speed the departing citizens or the armies of the state, and would be asked to accompany them and keep them, would be appealed to in their aspect of road-gods, as Mars was and as Janus was in Rome, when his temple-gate on the pomerium-line was opened each spring, and he was supposed to journey with the army and return with it.

It is singular that no critic of the Iguvian text has, I believe, seen that the double sacrifice outside and inside the gates was connected with the pomerium strip—was, in fact, a proof of its existence. There can be no question that Iguvium had a pomerium. The whole Iguvian ritual is based on the pomerial idea. It is the indispensable corollary of the *templum*. There

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[1] *De la véritable signification des monuments romains qu' on appelle arcs de triomphe,* in the Revue Archéologique, 1905. II, p. 216 ff.

could be no *urbs* without pomerium. Also, it is a well known fact that the entire circuit of a city's wall was consecrated. Now this presupposes that the land on which the walls stood was consecrated. If so, there must have been a line marking where this consecration began and where it ended. It must have extended on either side of the wall. It is only a question of how much. The *porta* of a city was, according to tradition, the only place where it was allowable to pass because the consecrated strip was here interrupted. That being so it seems as if any ceremony located at two points, one outside and the other inside every gate of a city must be referred to the only known topographical element at this point—the pomerial strip. All the cities of Latium and Etruria had both templum and pomerium. Whatever site was consecrated by augury and auspices had, of necessity, a pomerium to bound the consecrating formula.

In connection with the boundary line of the *urbs* of Iguvium and its three gates it is important to study the formula given in another part of the tablets for marking out the templum of the *arx*. Its importance is more than local: it affects our views as to the ground-plan of other early Italian cities, even of Rome. The formula shows that the *templum* of the citadel of Iguvium was undoubtedly *triangular*. I do not believe that such a thing as a triangular templum has thus far been even imagined. Yet it is incomprehensible how Bréal or any other student of the text could hold to the idea of a square templum for Iguvium. It was due, I suppose, to the orthodox hallucination that there could be no templum but a square templum. This fetish is so little founded that it is bound eventually to be abandoned, and I hope to contribute to its downfall. There were circular templa and triangular templa as well as rectangular templa. Varro expressly states (LL. VII, 6-13) that celestial and sub-terrestrial templa were of necessity circular, but that templa marked on the earth's surface could be made of any shape described in the consecrating formula. The proofs for circular templa I have already elaborated in another paper:[1] those for the triangular templum, besides the present Iguvian proof, will shortly appear. I may as well repeat here, however,

[1] American Journal of Archaeology, 1914 (XVIII), 3. pp. 302-320: Circular Templum and Mundus.

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this decisive passage in Varro from which we infer that surface templa could be of any shape, as it seems necessary in order to counteract the presupposition that the urban templum must be rectangular: *templum tribus modis dicitur: ab natura, ab auspicando, a similitudine; ab natura in caelo, ab auspiciis in terra, a similitudine sub terra.* After speaking of the whole hemisphere of heaven as the celestial hemisphere, he proceeds, as an example of the way a *templum in terris* can be marked out, to recite an early formula which describes a *triangular* form, a passage which has been as little critically studied and understood as has the text of the Iguvian tables which will now be examined.

I am preparing a study of the triangular form of templum, which was very commonly at the basis of the plan of the early cities of Central Italy outside of Etruria. I will here confine myself to demonstrating the fact for Iguvium alone.

First in order is the annual reconsecration of the templum of the *arx*. I will quote Buecheler's version of the text in *Umbrica:*

Templum ubi flamen versatur arcis piandae id stativum sic finitum est: ab angulo imo qui proxume ab ara divorum est, ad angulum summum qui proxume ab sellis auguralibus est; et ab angulo summo ad selles augurales ad urbicum finem; ab angulo imo ad aram divorum ad urbicum finem, et urbicis finibus utroque vorsum servato.

It will help if this description is turned into a diagram. The augural seat is at the upper corner, the *angulus summus,* of a

triangular area. The priest in marking out the templum must be imagined as facing eastward, and as marking the first side of the triangle on his left, both because the left was the lucky side[1] and because in the only other formula for the consecration of the templum from the arx, the above-mentioned archaic Latin ritual preserved in Varro, the priest is described as
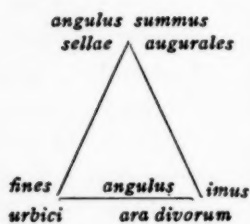
[1] I have treated on ancient Orientation and the lucky left in a paper read, Dec. 30, 1914, before the Archaeological Institute and American Philological Association, at Haverford. In it I show that the left was lucky and the right unlucky for Babylonians, Assyrians, Persians, Etruscans and Romans. To this list I can now add China.

running his first border line on his left hand. In the Iguvian ritual he does it by stretching his wand out toward the lower left-hand corner and then bringing it back toward himself in a motion from left to right. But in marking out the second line, that on his right hand, his motion is reversed. He starts it from his *sellae augurales* and runs it out toward the point where the *arx* joins the *urbs*. Why is this? Evidently because in this way he still keeps to the movement from left to right, which is the direction of luck, whereas had he done as before he would have followed the unlucky right to left movement. In marking the third or base line he does not simply continue the sweep of his arm from the *fines urbici,* but begins a fresh movement which starts at the opposite end, the *angulus imus* and ends at the *fines urbici*. The reason for this is the same: to preserve the lucky direction from left to right.

The first corollary of a triangular augural tabernacle or templum of the *arx* is that the larger templum of the *urbs,* whose consecration has been described above, was also triangular, with a gate probably in the centre of each side of the triangle.

The second corollary is that the boundaries marked out for the more elaborate ceremony of the lustration for the purification of the people of Iguvium were also triangular in shape. The general opinion seems to be that these boundaries were about a limited area, like the enclosure in the Campus Martius for the lustration of the Roman people. My own impression is that the area was large, was in fact that of the whole Iguvian canton, the ceremony and area corresponding to the *ambarvale* of the original canton of Rome, which was in charge of the Arval brothers. The phraseology used in describing the points through which the lustration boundary passed is made up of topographical terms suited to the country and not to the city.

This reflects some light on the course of the procession in the ceremony of the *amburbium* already described. In the sacrifices at the gates the procession must have moved from left to right, as this was the lucky direction, and have started at the gate on the left of the augural sea, as the left side was the first to be inaugurated. In a number of cases I have found that the plan of pre-Roman cities consisted of two triangles: a smaller one for the *arx* and a larger one for the *urbs,* joined by a narrow neck. I shall not discuss this at present nor

attempt to apply this scheme to Iguvium, but merely point out that the three gates and the triangular shape of the *arx* templum render the triangular scheme for the *urbs* practically certain, especially in view of what I shall now say of the other ceremony of the *lustratio populi,* where the triangular scheme is absolutely certain.

The territory is first cleared of all strangers by a proclamation three times repeated. Then the ceremony begins at a point called *ad Fontulos* or *in Fontulis.* Here three red or black boars are sacrificed to Serfius Martius. The procession then passes to its second station at a place called *ad Rubiniam,* where three red or black sows are sacrificed to Praestota Serfia Serfi Martii. This point seems to be connected with the main highway leading out of the Iguvian canton because two long prayers are here recited closing the road to strangers and opening it to Iguvians.[1] A libation is poured out to Fisovius Sancius. The procession proceeds to the third and last station at Trans Satam, where three calves are sacrificed to Tursa Serfia Serfi Martii. This ends the first act; but this grand tour is repeated twice to the same places: first to distribute the animals sacrificed and then to break the libation vases. In seeking to reconstruct the topography of the three stations it should be noted that the text connects Fontuli with the Porta Treblana and Rubinia with the Porta Tesenaca so that it is a fair inference to connect the third station at *trans Satam* with the third or Veian gate. It is also a fair inference that the roads leading out from the gates passed the cantonal boundary at these points. The placing of the entire ceremony under the auspices of Mars, or rather of hypostases of Mars, is a further analogy to the Roman form of *lustratio populi* and *ambarvale,* for Mars was the cantonal god and is now recognized as originally a god of the fields and of agriculture and only secondarily the god of war, as protector of crops and flocks.[2]

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[1] This inference is strengthened by the association of Mars with the Porta Tesenaca which the text connects with the road to *ad Rubiniam*: Mars was the *gradivus* deity *par excellence.*

[2] The *lustratio populi* in Rome in the Campus Martius, as distinct from the *lustratio urbis,* was probably a late symbolic partial substitute for the original *ambarvale.* The substitution took place, we may suppose, after the era of expansion was well under way, and the boundaries of the original canton of Rome—not more than five miles at its furthest—had become obsolete.

It seems evident, from a study of these various ceremonies which constitute the major part of the Iguvian tables that the sacred bounds were of the greatest importance. In the lustration ritual the bounds were marked by single instead of double ceremonies at the three points: the reason probably is that the cantonal boundaries were either natural streams or ditches, *fossae limitales,* and that the water or ditch constituted a single barrier. On the contrary the *amburbium* ritual, with its double ceremony at each gate, corresponds to the two borders of the pomerial strip, each border having its particular function and outlook, the one facing the *urbs,* the other facing the outside world. Each face corresponded to a definite divine aspect: one to the *pax deorum* of the home of the clan, where its gods were at rest with their people, the other to the dangers of the beyond, into which it was hoped that the members of the clan would go forth under the ægis of its gods.

There is, then, a basic concept necessarily underlying the choice of the gods to whom sacrifice was made at the outer boundary of the pomerium, and a choice also in the epithets given to the gods invoked under this particular aspect. What, now, do we find? We find that Jupiter *Grabovius* was invoked on the outer pomerium of Porta Treblana, Mars *Grabovius* on that of Porta Tesenaca, and Vofionus *Grabovius* on that of Porta Veia. Also, we find that the epithet "Grabovius" is not used as an epithet of these gods—or of any others in any different connection. It seems associated with the outer pomerium border only. It is interesting to remember that when the early Romans marched out each spring to war, they also considered themselves as accompanied by three gods: Jupiter, Mars and Janus. Jupiter conferred on the leader of the host his imperium; Mars *Gradivus* marched with the army; Janus made safe the way. With the substitution of Vofionus for Janus, we have a corresponding triad at Iguvium.

The logical conclusion is, in the first place, that the equation Grabovius = Gradivus is correct, but that its meaning was not, as Bréal thinks, connected with growth and productivity. It is Keller who is closer to the truth, and Grabovius denotes the marching, moving, aspect of the god to which it is applied. With the Romans it became restricted to Mars, but in the more archaic and conservative ritual of Umbria, it was applied to

any god invoked as an accompanying protector outside the boundary of the clan. The second conclusion is that the triangle was the form at the base of the *templum,* the *arx, urbs* and territory of Iguvium. This much seems incontestable. A third conclusion, while not so conclusively proved, is that the sacrifices outside and inside the gates mark the two borders of the pomerial strip and add to our evidence in favor of the strip form of the pomerium.[1]

<div style="text-align:right">A. L. FROTHINGHAM.</div>

PRINCETON, N. J., *December,* 1914.

[1] In justice to Professor Frothingham it must be noted that the proof failed to reach him in time.—B. L. G.

V.—THE SO-CALLED CALLIUM PROVINCIA.

The Thesaurus Linguae Latinae, under the caption callium provincia, Vol. III, p. 174, line 16 has the following examples: Curt. 3. 4. 5 qui callibus praesiderent. Tac. Ann. 4. 27 Cutius Lupus quaestor, cui provincia vetere ex more calles (*sic cod.*, Cales Lipsius) evenerant. Suet. Jul. 19 provinciae . . . minimi negotii, i. e. silvae callesque (del. Willems, le sénat de la république Romaine II 576, n. 5).

To judge from the plan of the Thesaurus, one would expect this to be a complete list of the references to the "province" during the period covered by the giant lexicon. In point of fact, it is rather more than complete, for the example from Curtius may at once be eliminated. The passage refers to the operations of Arsames, "qui Ciliciae praeerat", against Alexander the Great, and obviously can have no reference to a Roman provincia callium. After saying in 3. 4. 5 sed longe utilius fuit angustias aditus, qui Ciliciam aperit, valido occupare praesidio, "but it would have been far better to hold the narrow passes opening into Cilicia with a strong guard", the historian continues in 3. 4. 5 nunc paucis, qui callibus praesiderent, relictis ipse retro concessit, "as it was, however, the satrap withdrew, leaving a few men to hold the footpaths". Further comment seems superfluous, tempting as it is to enlarge upon the necessity of examining the context of citations and the exact meaning of the citations themselves.

On the other two passages (the one from Curtius is the peculiar property of the Thesaurus) various articles in our Dictionaries of Antiquities have been based, as well as sundry obiter dicta in our other books of reference. For example, in Daremberg and Saglio's Dict. des Ant. G. Humbert says, basing his statement on Suet. Jul. 19. 1, that the care of the forests and pastures regularly belonged to the censors, but was on that occasion assigned to Caesar and his colleague.

H. Stuart Jones, in his Companion to Roman History, p. 313, remarks: "the broad trackways by which they (the herds

of cattle) marched—e. g. from Reate and Samnium to Apulia
—were called calles publici and the maintenance of order was
assigned to officials of high standing: praetors, quaestors,
even at times a consul". That this alleged assignment of the
"province" to a consul is based on Suet. Jul. 19, and to
quaestors on Tac. Ann. 4. 27, seems obvious. What the
authority is for saying that it was assigned to praetors is not
apparent, and the statement lacks support, unless the author
has access to material not available to the compiler of the
article callis in the Thesaurus, and to the writer.[1]

E. S. Bouchier, in his revision of Arnold's Roman Provincial
Administration, p. 49, makes this statement: "when, for in-
stance, they (the senate) expected the election of Caesar, they
took care to provide beforehand that he and his colleague
should have the unimportant province of the roads and
forests". Since the two books last mentioned have appeared
within the last year or two, the writer's dissent from the
views which he has quoted from them seems to offer sufficient
justification for a brief discussion of an old problem.

Mommsen took Suetonius's provinciae in a different sense,
and in his History of Rome, IV. 512, Eng. trans. says: "con-
stitutionally it devolved on the senate to fix the functions of
the second consular year of office before the elections of con-
suls took place; accordingly it had, in prospect of the election
of Caesar, selected with that view for 696 two provinces in
which the governor should find no other employment than the
construction of roads and other works of utility". The
writer of the article Callium provincia in the third edition of
Smith's Dictionary of Antiquities, citing Mommsen, maintains
that the words of Suetonius can hardly have the meaning
which the great German scholar gave them. This is true
enough, if Mommsen's language be regarded as a literal
translation. It is, however, a very good paraphrase and gives
the true sense of the Latin, which may be rendered literally

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[1] Mr. Stuart Jones writes me that his statement was based on line 27
of the Lex Agraria (C. I. L. I. 200) and Mommsen's commentary. I
am indebted to him for reference to the exhaustive article of Grenier,
La transhumance des troupeaux en Italie, in Mélanges de l'école fran-
çaise de Rome, 1905, pp. 293 ff., which however does not affect my
conclusions.

as follows: "provinces involving little activity, that is mere woods and pastures".

That Mommsen is right in regarding the provinciae as districts to be governed by Caesar and his colleague at the close of their term of office, and not as a sphere of duty assigned them during their consulate, would seem too evident to warrant discussion, were it not for the persistence of contrary opinions. As a matter of fact, no consular function which could be indicated by silvae callesque ever existed, so far as we know, and the greater number of those who take provinciae in that sense assume that a sphere of duty properly belonging to a quaestor (or according to Humbert, to the censors) was, for the purpose of humiliating Caesar, assigned (in advance of the elections) to him and his future associate in office.

Now, while the senate would be quite within its rights in assigning to the consuls provinces to govern which demanded no great amount of protection or pacification (in the Roman sense of the term), and while they were required by the lex Sempronia to make the appointments before the election took place, it seems inconceivable that the senators should have gone so far as to assign the consuls duties to be performed during their term of office which properly belonged to a quaestor; duties too, which would have obliged them both to be absent from the city during their incumbency of the consulship. It is also at least an open question, whether duties of that kind were ever assigned in advance of the elections.

Moreover, since the "provinces" in question were assigned to *both* consuls (futuris consulibus), while the so-called callium provincia was the function of a single quaestor, we are obliged to conclude: either that silvae callesque in the passage from Suetonius is synonymous with calles, and that the duty of one quaestor was assigned to two consuls; or that there was besides the callium provincia a silvarum provincia (to which we have no reference anywhere), and that one of these was assigned to Caesar and the other to Bibulus.

Furthermore, if the senate actually had the audacity to make such an appointment, it was wholly and immediately disregarded, for both Caesar and Bibulus remained in Rome, and the former performed all the usual duties of a consul, to

say the least, during the active administration of " Julius and Caesar " (Suet. Jul. 20. 2).

We therefore seem compelled to accept Mommsen's view of the nature of the provinciae as the correct one. This being so, silvae callesque is either a gloss, as Willems thought, added as a further definition of provinciae minimi negotii by some scribe, who first misunderstood the meaning of provinciae and then confused a quaestor's sphere of duty with a consul's; or, with greater likelihood, as I tried to show in P. A. P. A. XLV. xlvii ff., is a colloquial term, "mere woods and pastures ", applied to such provinces as Mommsen believed Suetonius to mean, probably by Suetonius himself, possibly by the copyist of some manuscript earlier than any of those now in existence, all of which contain the phrase.

It may be urged that even if we give the words of Suetonius this interpretation, we are still obliged to admit that the action of the senate was set aside. That is true enough, but it was not done until Caesar had won the support of Pompey and Crassus and was backed by an influential father-in-law and son-in-law (socero igitur generoque suffragantibus, Suet. *Jul.* 22. 1). Furthermore, it was done constitutionally and by due process of law: initio quidem Galliam Cisalpinam Illyrico adiecto *lege Vatinia* accepit; mox *per senatum* Comatam quoque (Jul. 22. 1).

Of course provincia has the general meaning of a function or sphere of duty, as well as that of a Roman province. It is frequently so used metaphorically and usually in a jocose sense: for example, Plaut. Capt. 474 ipsi obsonant, quae parasitorum ante erat provincia; Cic. ad Att. ap. Suet. Gramm. 14 sed mihi solitudo et recessus provincia est. Provincia is also used literally in this general sense, but for the most part (so far as we may judge from our incomplete lexical material) at a comparatively early period. It is frequently applied in this sense to the assignments to the consuls of military commands: for example, Livy 26. 22. 1 consules cum ambo Apuliam provinciam haberent; Livy 2. 40. 14 consules T. Sicinius et C. Aquilius. Sicinio Volsci, Aquilio Hernici (nam hi quoque in armis erant) provincia evenit. Of other functions, and at later times, this use of the word seems to be very rare; at least, the handbooks and the references upon which their ac-

counts are based (as well as the lexicons) give us little or no
information about the duties of the various officials under the
title of provinciae. I would hazard the conjecture that, except
in its metaphorical sense, provinciae meaning a function is al-
most wholly confined to the military appointments just men-
tioned and the " Italian quaestorships " to be discussed below ;
and that, except in accounts of the earlier history of Rome,
its use as the designation of a Roman province almost wholly
supplanted its other meaning. Suetonius, who frequently
uses the word, always employs it of a district, except in the
quotation from Cicero cited above, and in Claudius 24. 2.
(cited below). The former of course does not enter into the
question, and the latter seems in reality to be no exception to
his regular usage; for while provincia Ostiensis and provincia
Gallica are peculiar in referring to districts in Italy (Cisalpine
Gaul was incorporated into Italy by Augustus), the meaning
of the phrases taken by themselves is clearly " the Gallic pro-
vince ", for example, rather than " Gallic functions " or " a
Gallic sphere of duty ". But whether this be so or not, Sue-
tonius offers at most but one exception to his regular employ-
ment of provincia in the sense of a district, and the usage of
Tacitus appears to be equally consistent.

Although provincia seems to be rarely, if ever, applied to
the various duties of the several Roman magistrates, we are
well informed about them, and it seems safe to say that we
know of no function of the consuls, praetors, or censors which
could with any propriety or naturalness be spoken of either
as silvae callesque, or as calles.

It remains to consider whether there was a quaestor's
" province " called callium provincia. Since the silvae calles-
que of Suetonius may be eliminated with almost as much con-
fidence as the callibus of Curtius, we have left in testimony
to the existence of such a function only the passage in Tacitus,
Ann. 4. 27. Here Lipsius proposed to read Cales instead of
calles, and the emendation was accepted by Mommsen and
others, who believe that the four " Italian quaestors " appointed
in 267 B. C. were quaestores classici and that their headquarters
were at Ostia (provincia Ostiensis), at Ariminum (provincia
Gallica), at Cales, and at some other place unknown. This is
stated positively by Mommsen, Staatsr. 2. 571, (except that he

thought that the headquarters of the Gallica provincia might have been Ravenna), by Schiller in Müller's Handbuch 4. 1. 85 (also with a reservation about the headquarters of the Gallica), in Di Ruggiero's Dizionario Epigrafico, s. v. Cales, and elsewhere. On the other hand, the subject of the " Italian quaestors " is handled with the greater caution which the nature of the evidence makes advisable by Abbott in his Roman Political Institutions, p. 209.

As a matter of fact, our information about these quaestors is very scanty. Their supposed title of quaestores classici is supported only by Joh. Lydus, de Magistr. 1. 27, and as Abbott says, the nature of their functions is not perfectly clear. Suet. Claud. 24. 2, collegio quaestorum pro stratura viarum gladiatorum munus iniunxit detractaque Ostiensi et Gallica provincia curam aerari Saturni reddidit, justifies the conclusion that one of them was stationed in Ostia and another in Gaul. These provinces were called respectively Ostiensis and Gallica, and it is reasonable enough to suppose that the headquarters of the latter was at Ariminum or at Ravenna. Mommsen (Staatsr. 2. 571) assumes that a third quaestor had charge of a district in southern Italy, extending far enough eastward to include Brundisium, and that its headquarters were at Cales, " die älteste lateinische Colonie in Campanien und als diese Quästuren gegründet wurden, ohne Frage die römische Hauptstadt Campaniens ". Unfortunately the only support for this opinion is Lipsius's emendation of the passage in Tacitus, and while the change from calles to Cales is a comparatively easy one, it is a question whether it is necessary or warranted. Furneaux, in his edition of the Annals, says: " the manuscript text has little to recommend it, for the passage in Suet. Jul. 19 has no reference to Italian quaestorial districts or to any one definite locality ". With the last part of this statement the writer's opinion is fully in accord, and it is certainly true that no support for calles can be derived from Jul. 19. But after all, since the manuscripts are unanimous for calles, the burden of proof surely rests on those who would change the reading, and the evidence for a quaestorial "province" with its headquarters at Cales is hardly strong enough (if it exists at all) to justify substituting Cales for the traditional reading.

Orelli, in his edition of Tacitus, makes a statement which is also a pure assumption, but is no less probable than that of Mommsen, with the additional advantage of demanding no change in our text. He says that calles Italiae was a term applied to the wooded district extending from the Campanian frontier (a tergo Campaniae) towards the Adriatic, and that because of the revenue yielded by the herds of cattle which were pastured there a quaestor was put in charge of it. His first statement is apparently confirmed by Cic. pro Sest. 12 neque umquam Catilina, cum e pruina Appennini atque e nivibus illis emersisset atque aestatem integram nactus Italiae callis et pastorum stabula praedari coepisset ... concidisset. The second too perhaps derives some support from the fact that in imperial times the district in question was administered by the emperor's procurators; see Pelham, Class. Rev. X. 6, whose statement, however, that Suetonius tells us in Claud. 25 (=24) that Claudius substituted procurators for the quaestor, does not seem to be quite exact, although such a supposition is probable enough. Hence to assume the existence of a quaestor's "province" connected with the calles Italiae does not seem unreasonable, directly supported though it is, only by the single reference in Tacitus. That there were quaestorial functions to which we have merely an occasional and cursory reference appears from Cic. Vatin. 12 in eo magistratu (= quaestura) cum tibi magno clamore aquaria provincia sorte obtigisset, missusne sis a me consule Puteolos, ut inde aurum exportari argentumque prohiberes?

Granting the existence of a "province" connected with the calles Italiae, we can hardly suppose that it was officially known as callium provincia. To judge from the two provinces of the kind which Claudius mentions, it is more probable that it was designated by a geographical adjective referring to the district as a whole, like Gallica provincia. With the elimination of Cales we have no certain example of a province named from its chief city; for Ostia, with its extensive commerce in grain and other commodities, could furnish employment enough to occupy the entire time of a quaestor, and I am not aware that we have any evidence that the provincia Ostiensis extended beyond the limits of Ostia itself.

Even if a geographical adjective was not used, we should at least expect in an official title something more definite than callium provincia. That Tacitus, however, might refer to such a province by the general descriptive term calles, instead of by its full title, is indicated by the passage from Cic. Vatin. 12, where aquaria provincia can hardly be the official designation of the "province" in question. It is a descriptive term, just as the office of the curatores aquarum might conceivably be referred to as aquaria cura, instead of cura aquarum (Suet. Aug. 37). In the same way the "province" mentioned by Tacitus, whatever its official designation may have been, is referred to, with his usual conciseness, simply as calles. Its full title may have been, in default of an adjectival designation covering the entire district, Italiae callium provincia; or more probably, calles Italiae, without provincia.

It may be added for the sake of completeness that Dio, 55. 4, tells us that the "Italian quaestorships" were established by Augustus, which is commonly regarded as an error; and that in 60. 24 he says that all these quaestorial provinces, as well as the Ostiensis and Gallica, were abolished by Claudius. The second statement is confirmed by Tacitus's vetere ex more, which indicates that the office no longer existed in his time, while evenerant seems to show that the assignment was made by lot.

To sum up: it seems beyond doubt that the provinciae of Suet. Jul. 19 are not functions, but districts outside of Italy, and as Furneaux rightly says, there is absolutely no parallelism between the passages in Suetonius and Tac. Ann. 4. 27, except the purely accidental occurrence of calles in both. The evidence for a province having to do with the calles Italiae is at least as strong as that for one with its headquarters at Cales, and we are therefore not justified in emending calles to Cales in Tacitus. We have no evidence at all for a province officially known as callium provincia, a term which, it will be observed, Tacitus himself does not use. It is highly probable that a quaestor was assigned the calles Italiae under some more appropriate official designation, and that Tacitus refers to this "province" when he says: cui provincia vetere ex

more calles evenerant, that is, "to whom as his sphere of duty the calles Italiae had been allotted ".

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NOTE.—Since the above was written, I have noticed the following reference to the subject in Professor Tenney Frank's Roman Imperialism, p. 335: "he (Caesar) refused to accept the province over 'highways and pastures', that the senate assigned to his proconsulship ". I am glad to find that Professor Frank agrees with me in referring silvae callesque to the proconsulship of Caesar, and not to his consulship. The rest of the sentence is not clear to me. Does it mean that the so-called callium provincia was given to Caesar instead of a province outside of Italy? If so, I have given reasons for doubting this, although that of absence from Rome of course would not apply to a proconsular appointment. Or does it mean a province *consisting of* silvae callesque, which is precisely my own opinion? In either case, "highways and pastures" is not an accurate translation of silvae callesque. Calles may mean pastures, but silvae surely does not mean highways. The only reading which could possibly be translated "highways and pastures" is semitae callesque, taking semitae as a slighting way of referring to viae. But this reading, so far as I know, is found nowhere except in Harper's Lexicon, where it is incorrectly referred to Aug. 19, instead of to Jul. 19. It has absolutely no manuscript authority, nor is it, so far as I am aware, the conjecture of any scholar of repute.

REVIEWS AND BOOK NOTICES.

Sokrates: von Adolf Busse. Berlin, Reuther & Reichard, 1914. Pp. X+248.

This volume is the seventh of the series "Die grossen Erzieher" issued under the general editorship of Professor Rudolf Lehmann. As such it is ostensibly most concerned with Socrates as a pedagogue; but in fact its author has endeavored to appraise the personality, character and teachings of his hero in such a manner as to represent them fully and as a whole.

The plan of the work is well conceived, as the table of contents and the exposition sufficiently prove. After an introduction dealing with the sources for a knowledge of Socrates, the matter is presented under three general heads, concerned respectively with the life and personality, with the teachings, and with the trial and death of Socrates. It is hardly necessary to enumerate the several captions of the subdivisions. There is no index, but the carefully constructed analytical table of contents enables one to find what may be required. References to particular details will not be missed because the author has contributed little to the interpretation of special passages in the sources. The style of the author is easy and lucid, and the book as a whole gives one the impression of a high-minded and sympathetic teacher, who has caught not a little of the spirit of the great master and is endeavoring by this work to convey to others the insight into his heart and teaching which prolonged study has yielded.

Concerning the matter which our author undertakes to set forth the judgment of the critic cannot be in all respects so favorable. It has been already said that Dr. Busse has not contributed much that is new to the interpretation of our sources; indeed the general character of his book hardly called for that, since it was clearly designed for a somewhat general public. From the introductory estimate of the sources to the end of the book there is comparatively little discussion of moot-points, and where such questions arise a reference to more detailed expositions is made to suffice the reader. The character of the works thus cited is such that one feels at times that our author is not acquainted with the best special literature of his subject. The best portions of the book are those which, like the concluding chapters, call for the insight into human nature and for the sympathetic appreciation of the

stalwart character of Socrates, which a teacher of high ideals, used to reading the Apology, Crito and Phaedo with boys of the higher forms, would naturally cultivate.

Other parts of the book are notably weak. This is particularly true in respect to whatever concerns the intellectual and historical background of the thought and life of Socrates. The discussion of the Sophists is perhaps the weakest part of all, in spite of a very real appreciation of the aims of the leaders. It might be thought that when all is said an error here, even granted that it was an error, could not be regarded as of much importance, since after all the central figure of the picture is Socrates, and every other must therefore be secondary or unessential. In real life and generally in history this point of view is justified, and no doubt Dr. Busse would hold that it is applicable in the present instance also. I shall try to show that it is not so, and shall develop this thought and a few of its consequences in lieu of a detailed review of the book here under notice for two reasons. First, I am on a vacation with nothing but Dr. Busse's work at hand, and could not therefore profitably discuss minutiae, and in the second place I hope thereby to indicate in a positive way the central problems in the interpretation of Socrates.

I cannot here undertake a discussion of the vexed question as to who among the three leading authorities—Plato, Xenophon, Aristotle—is most to be trusted. Dr. Busse's theory and practice—neither quite clear—do not altogether agree on this all-important point. Fortunately for me, Dr. Busse in the main takes the view which for the nonce at least I also prefer to take, to wit, that we must make Plato's Apology our point of departure. The view to be obtained in this way, if it be sharply defined, is far from simple and raises some questions of extreme importance. No one who chooses to follow this way should fail to realize how revolutionary the conclusion may be; for hitherto, if I mistake not, no modern scholar has unreservedly taken this line. And it is not at all surprising that this should be the case; for modern thought about Socrates is dominated by the conception that he was first and chiefly a philosopher, a view which could never be reached by a plain, intelligent interpretation of the Apology. The first requisite on this view is then the interpretation of the Apology. After that has been settled, the question will arise whether the Socrates thus disclosed is the real Socrates or a figment of Plato's imagination. This question I cannot now discuss; but it will be seen that I believe Plato seized upon the central truth and that his reading of his master's character, different as it is from that of others, differs from theirs only in being more profound and in offering the best clew to the unravelling of the tangled skein of evidence about that hero of the spirit.

22

No one can read the Apology without perceiving that two passages afford the key to the whole, considered as a declaration of the inner life and ultimate purpose of Socrates. The first is that in which he relates the story of Chaerephon's question addressed to the Delphic oracle, of its response, and of the quest on which Socrates entered in consequence thereof; the second is that in which Socrates defines the scope of the divine mission to which he thereby believed himself called, and to disobey which must be for him the final negation of God. To make light of these passages or to fail to interpret them aright is utterly to miss the clew which Plato offers. Dr. Busse speaks of the latter as singular, and quite misapprehends its significance; this is fatal to his interpretation of the life-work of Socrates. He misses also the relation of the quest of Socrates to the response of the oracles which called it forth, and hence discovers its bearings only on the subsequent unpopularity of Socrates.

The oracle declared that there was none more σοφός than Socrates. This dumbfounded Socrates, as well it might. *He* a σοφός? What might the god mean? He was not conscious of being σοφός at anything. Could this very sanity of his, which flew in the face of a divine oracle and refused to discover any wisdom in himself, itself being a kind of wisdom? There was Myson of old, of whom Hipponax related that the Delphic oracle had declared him of all men most σώφρων. Undoubtedly σωφροσύνη was a form of σοφία, and Socrates then or later recognized in σωφροσύνη the ideal compliance with the injunction of that same oracle, γνῶθι σαυτόν. Here were three texts of scripture which might be harmonized on this view. However, the question must not be thought so easily disposed of. What really was a σοφός? Socrates canvassed the conceptions of the Greeks, just as later Aristotle, before defining a term, asked the question ποσαχῶς λέγεται; This is the meaning of the three classes of men, to whom Socrates addressed himself in quest of a σοφός, the statesmen, the poets, the artisans. To all of these the Greeks applied the term σοφός, and they practically exhausted the list. The statesmen as σοφοί were numbered in the glorious company of the Seven Sages. Here again the Delphic oracle, according to ancient legend, had had a hand in assigning the golden tripod; but it is doubtful whether this legend existed in the time of Socrates. At all events the oracle was not credited with actually designating the σοφώτατος. the decision was to be reached by an informal ballot among the candidates, on the principle (attributed I believe to Xenophanes and Empedocles) that it takes a σοφός to catch a σοφός. This fact rules the legend of the golden tripod out as a possible factor in the thought of Socrates, and inclines me to think it of later origin. Nevertheless the σοφός as a states-

man was an old conception in the days of Socrates, and needed to be considered. Among the bards and singers Homer, Hesiod and Orpheus were accounted σοφοί, each with his peculiar σοφία; and the artisans too were credited with σοφία, each after his kind. Socrates, in search of a σοφός, found among the contemporary representatives of these canonical classes no common trait which could be praised as σοφία and claim the approbation of the god of Delphi: rather he found as their common characteristic a vice which sinned against the god's injunction, γνῶθι σαυτόν and the σωφροσύνη which had received his commendation. If Socrates found in himself that virtue which he had missed in those among whom he had vainly hoped to find σοφία, he was so far forth entitled to think of himself as in the thought and definition of the god a σοφός. But what a revelation of himself, of the mind of the god, and of his own duty lay in that act of self-discernment! Plato represents this insight as the turning-point in the career of his master.

It is noteworthy that in his quest for a σοφός Socrates does not include the philosophers and sophists. Plato is at great pains to distinguish between Socrates and all these, but does this in a wholly different connection. They were evidently not in the thought of Socrates when he canvassed the meaning of the god in pronouncing him a σοφός. The references and allusions to them, and there are many, are found in the portion of his defense, in which Socrates seeks to save himself from being confused in the minds of his accusers and of the public with the pretentious aspirants to the title of the σοφός. To Socrates their arrogance in laying claim to a title like this must have sufficed to exclude them from his field of view while he was in search of a σοφός; but Plato felt the need, nevertheless, of marking off the lines in such a way as to make clear the difference between them.

But no definition could be complete in the Socratic sense which rested upon exclusion. The οὐσία of anything, consequently of the σοφός, is defined positively by its function or purpose. The purpose and function of Socrates, as the σοφός of god's election, is expressed in his divine calling and mission. Here we see the necessary connection of the two passages of the Apology, which, as we have said, furnish the clew to Plato's reading of his master's character. The nature of that mission cannot remain for a moment in doubt when one squarely faces the words of Plato. Socrates regarded himself as an instrument called in the providence of god to rouse his people from the supine slavery to the world and the flesh, to a sense of kinship with the divine and a life in keeping with the possession of a soul eternal and of infinite possibilities. This means, of course, that Socrates was not a philosopher, but a

preacher of righteousness and of religion. To be sure, the word preacher suggests erroneous connotations. Socrates did not appeal to the multitude, but to the individual soul; but this is a question of method, not of aim. Believing as he did (and was he not right?) that all instruction, all communication of ideas and ideals, is dependent upon the active preparation of the learner, who thereby discovers or creates for himself the truth he would apprehend or the ideal he would conceive, he could have no recourse to a Nürnberger Trichter of whatever sort, but practiced the maeeutic of the Socratic method. The manner of his quest, the seemingly unfeeling probe of the elenchos, were but the missionary's Busspredigt preached to the sinner in the retirement of some nook to prepare his soul for the conception of the higher ideal which should form the basis of a new life. As the preacher does many things to win souls to the kingdom, so Socrates also followed many a tack, discussed many a theme, in pursuit of his mission.

There is another point that should be considered. When in the Apology Socrates suggests that a fitting requital for his manner of life would be his maintenance in the prytaneum at public charges, and refers in deprecation to the reward of athletes, there can be no doubt that he had in mind the scornful rebuke of Xenophanes, who asserted the superiority of his σοφία. This reminds one of the acts recounted of Jesus in the Gospels as performed that the scriptures might be fulfilled. Socrates in speaking as he does speaks as the representative of a tradition. Other points of the same general character are not wanting. One of the functions of the σοφός was that of the prophet. The prophecies of Thales, Anaximander, Pherecydes, and Pythagoras attest the ideal. So also do the functions of the 'priests' in the characterizations of the Indian sages, reflecting the fourth century's ideal of the σοφός, which are found in the historians of Alexander. Hence Socrates must possess mantic powers, as in his δαιμόνιον, in his prophetic dream in the Crito, and in his prophecy to the jurors at 'the sunset of life.' The σοφός is a poet and a musician: so when Socrates dreams again and again that he is to practice μουσική, lest he leave undone any of the things done by the σοφός, he versifies Aesop, and takes instruction in music of Damon and Connus. How much of this is historically true, how much was related of him merely that "the scriptures might be fulfilled", we shall never know; but the relation of these things to the 'Messianic' conception of the Greeks, their ideal of the σοφός, is obvious. In the same way Socrates links his own quest with another Messianic hero of the Greeks, when he likens his labors in the service of the god, to those of Heracles. Indeed, we shall never read the character of Socrates aright until we learn to read it in the light of the whole Greek tradi-

tion of the σοφός, which is the Greek contribution to the 'Messianic' ideals of the nations. Nor must we forget the Aeschylean Prometheus, the physician who could not heal himself, the martyred friend of man, who stubbornly clung to his conception of right, undismayed because immortal. The unyielding determination of Socrates to do the right even unto martyrdom, inspired by the conviction that he too (his soul) was immortal, has much of the titanic in it, and was doubtless in part inspired by the same ideals, if not by the character of this other ideal σοφός.

It would carry us too far to dwell on the decisive influence of the character of Socrates on the subsequent career of the ideal σοφός in Greek thought. Suffice it to say that the σοφός of all the later schools presents in various refractions the ideal portrait of him as depicted by one or the other of his disciples.

There is one more question of importance which must be answered before the character of the σοφός, as seen in the Platonic reading of Socrates, can be fully appreciated. That is the precise relation of it to the ideals of the σοφός embodied in the Ionian philosophers on the one hand and in Pythagoras on the other. To pursue this theme here would, however, lead us too far. The essential characteristics of the σοφός, as represented by these types, have not yet been clearly set forth by any scholar, and the task is too serious to be undertaken in a sketch like this. Particularly in the case of Pythagoras the question arises again and again whether Socrates is the copy or the model, a question which only the most careful historical research can hope to answer. In the writings of Plato much is made of the opposition of Socrates to the Sophists, and this also constitutes a serious problem, since the σοφιστής purported to be a σοφός, and the ideal of the Sophist must therefore be set into relation to the whole tradition of the σοφός. That in the main the Sophist stands most closely related to the Ionian σοφός must be obvious even to the superficial observer; whereas the Socrates of Plato's Apology finds his closest analogue in the Orphic religionist. Thus is raised anew the question as to the truth of the traditions which represent Socrates as the disciple of a Sophist, like Prodicus, of an Ionian, like Archelaus, or of a Heraclitean, like Cratylus. And here one would have to consider the sketch of the development of Socrates given by Plato in the Phaedo.

But I must now leave these questions for others or for another occasion. They are only indirectly concerned with Plato's Apology, but their bearing is obvious as is also the need of far more careful consideration of them than they have hitherto received. Only when the ideal of the σοφός has been traced through the length of the Greek tradition can we hope

to interpret the life and character of Socrates and give to them their proper setting. In this endeavor one must begin with Plato's Apology, but the other dialogues of the great disciple must be brought into relation with it.

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Oidipus. Geschichte eines poetischen Stoffs im griechischen Altertum. Von CARL ROBERT. Erster Band. Mit 72 Abbildungen. 587 pp. Zweiter Band. Anmerkungen u. Register. Mit 17 Abbildungen. Pp. 203. Berlin, Weidmannsche Buchhandlung, 1915.

My chief companion for the summer months of the current year—I had wellnigh said, 'the long vacation'—was to have been Robert's Oidipus, and for edification and illumination I had chosen not unwisely. In his masterly hands the story of Oidipus, which most persons are fain to accept in the wonderfully articulated version of Sophokles, has become an encyclopaedia of mythology, a study of topography, a history of epic and dramatic composition, a gallery of vase-paintings. There are two volumes, and nearly 800 pp. When Henry James's Portrait of a Lady was on its winding way through the Atlantic Monthly, Aldrich, the editor of the Atlantic, is reported to have written to the author, 'Call you this a Portrait or a Panorama?' And a similar question rises to the lips when one surveys the number of pages. 'Call you this a Legend or a Library?' It is not so bad as Joël's book on Sokrates, but still not light summer reading. However, the table of contents justifies the amplitude and there is diversity enough to keep up the interest. I, Die Kultstätten des Oidipus—Eteonos-Sparta—Attika; II, Die Sphinx; III, Oidipus, König von Theben; IV, Eteokles u. Polyneikes u. der Bruderkrieg; V, Das Epos; VI, Das Drama; a) Die Thebanische Trilogie des Aischylos; b) Der erste Oidipus des Sophokles; c) Der Oidipus des Euripides; d) Die Antigone des Sophokles; e) Die Antigone des Euripides; f) Die Phoinissen des Euripides; g) Der zweite Oidipus des Sophokles; VII, Oidipus bei den übrigen Tragikern u. in der Paradoxographie; VIII, Oidipus in der Mythographie. Beilagen: I, Die Aigiden; II, Der Kolonos Hippios.

Any one of these chapters would yield matter for a review by a specialist and I recognize the hopelessness of my original plan of doing for Robert what I have done for others and giving a summary of this monumental work. Summaries have their use as well as criticisms; and monumental the work is in a double sense, for it is dedicated to the memory of Tycho

von Wilamowitz-Moellendorff, 'who fell on the 15th of October, 1914, before Ivangorod in the hour of victory'. The blood that oozes from the sponge which has wiped out so many noble lives stains every page of the times in which we live and suffer—indeed, I might say, live to suffer—and many pages of memory as well.

As I have just intimated, the magic wand of Sophokles has conjured up the stately dome of an Oidipodeia which it is impossible to think away. We all know that Sophokles has taken liberties with tradition. So have the other dramatists. But his is the version that abides in the memory. No matter what Father Homer may tell us, what Aischylos, what Euripides, we decline to be disillusioned. Homer makes out that the wife of Oidipus was Epikaste but it was Iokaste, not Epikaste that said: ἅλις νοσοῦσ' ἐγώ. Oidipus must not die in his bed. He must be spirited away—like so many heroes, sacred and profane. Euripides' variations are interesting and I am personally grateful for the words he has put in the mouth of his Iokaste, as she addresses her elder son. I have quoted them often of late years:

> ὦ τέκνον, οὐχ ἅπαντα τῷ γήραι κακά,
> Ἐτεόκλεες, πρόσεστιν, ἀλλ' ἡμπειρία
> ἔχει τι λέξαι τῶν νέων σοφώτερον.

But my Iokaste had hanged herself, before the hair of the heroine had blossomed white, before her children were old enough to understand the pollution that had come upon the house of the Labdakidae. We have all known for years that many scholars had vied in proving that the dread Sphinx of worldwide renown was merely a terrible Phix of local fame, but what is proved to one who refuses to believe? In like manner Kolonos was the burial-place of Oidipus and no other, although the exact spot is not known. As was the case with Moses, 'no man knoweth of his sepulchre to this day'. A place was prepared for him as a great fish was prepared for Jonah. But, however completely one may be under the spell of Sophokles, there is no denying that Robert has shed a flood of light on these old questions, that he has opened vast vistas through the jungle of tradition, has set out a whole plantation of new hypotheses and, as was to be expected of the author of 'Bild und Lied', has brought a store of archaeological knowledge to bear on the wide range of his researches; and if in the limited space allotted to reviews I cannot do justice to this important work, I hope I shall be pardoned for jotting down some points new or old that have interested me and may interest others :—

The very first chapter deals with the locality of the cult of Oidipus, of the place of his burial. The genius of Soph-

okles has identified it with Kolonos. But Robert questions whether that can possibly have been a Boeotian legend. That Oidipus was banished, that he wandered in the wilderness was doubtless a Boeotian tradition but not the burial in Attica: and Robert marshals the evidence in favor of Eteonos, a vanished town on the northern slope of Kithairon, where there is said to have been an actual cult of Oidipus and he thinks that this theory gives a special point to O. R. 1451:

ἀλλ' ἔα με ναίειν ὄρεσιν, ἔνθα κλῄζεται
οὑμὸς Κιθαιρὼν οὗτος, ὃν μήτηρ τέ μοι
πατήρ τ' ἐθέσθην ζῶντι κ ύ ρ ι ο ν τ ά φ ο ν.

The Spartan site is rejected as originating in the settlement of a noble Boeotian family in Sparta. In this version Oidipus is too much in the background. Of the Attic sites the chasm beneath the Areopagus does not open up as the burial-place of Oidipus until the fifth century. The Kolonos story is older but lacks real warrant. What is common to all these localities is the neighborhood of a temple of Demeter and the connexion with the Erinyes who are of the same nature with the Earth-goddess. <Alas for the interpretations of mythology! Time was when some of us were taught to regard the Erinyes as storm-clouds and to read a deeper meaning into the dying speech of Aias:

καλῶ τ' ἀρωγοὺς τὰς ἀεί τε παρθένους
ἀεί θ' ὁρώσας πάντα τὰν βροτοῖς πάθη,
σεμνὰς Ἐρινῦς τανύποδας κτέ.

The only way to reconcile the two interpretations is to suppose that the Erinyes, melted into rain, became Eumenides and being bottled up in underground cisterns were thenceforth associated with Demeter.> An idle story about the final burial of Oidipus in Thebes is dismissed in a few words. The repatriation of bones is one of the most familiar transactions in history from the earliest times to this day. No wonder then that it is one of the stock stories of fables.— There is, Robert repeats with emphasis, no really old cult of Oidipus <as I should put it, no cult of the father of Eteokles> save at Eteonos. Here is the sum of the whole matter. Oidipus is a chthonic hero of the Demeter cycle, a nature myth. The mother of Oidipus is the Earth-goddess, who is wedded to her own son. Oidipus shares her couch as we shall all share the couch of the Great Mother. Kithairon was the cradle of Oidipus. On the Eteonos theory, Kithairon was to be his grave. He slew his father as the New Year kills the Old Year. <Nowadays in accordance with humanitarian principles the sentence

is commuted to perpetual banishment, but we still kill Time and Time kills us, 'ce vieux Parthe, qui nous tue en fuyant'.>

Of course, I am somewhat, though not bitterly disappointed that Robert has taken no notice of my interpretation of the famous riddle of the Fourth Pythian of Pindar. True, he cites P. 4, 263, γνῶθι νῦν τὰν Οἰδιπόδα σοφίαν, to prove that Oidipus was renowned for his wisdom, but one naturally looks for something deeper than that, something more apposite than that, and I have tried to shew that the exile of Oidipus and his burial in a strange land have a bearing on the story of Damophilos, whose name, by the way, might serve as a surname of Oidipus himself. There is no story, not even that of Theseus, in which the love of suzerain for subject and subject for suzerain rises to a higher tide than in the story of Oidipus. The Kolonos story may have been known to Pindar, even if it is not mentioned by Aischylos. Assuredly it seems more natural that the benefits accruing from the burial-place of Oidipus should be remembered in connexion with a great city like Athens than with an obscure village on the Northern slope of Kithairon.

That the name Sphinx 'the strangler' was developed by popular etymology out of Phix, a local monster, and her form derived from an originally Egyptian type is, as I have just said, an old theory to which Robert holds, despite recent attempts 'to becloud the simple facts'. The illustrations to the chapter are fascinating and those that represent the Sphinx as carrying off her victims will linger long in the memory. The diabolical smile of the Sphinx and the uncompromising position of the figures might serve to illustrate the story of the three old women in the Ecclesiazusae, or else the Vampire of Rudyard Kipling. Originally the Sphinx was overcome by heroic methods, with heroic weapons. The Sphinx of the riddle was later. The poetical form of the enigma is traceable to an epic poem, some Oidipodeia or Thebais. It cannot be older than the age when the riddle poetry was at its highest. The answer to the riddle is the guesser himself. It is as Robert says a coarser and more concrete form of <the heaven descended> γνῶθι σαυτόν.

The benevolent rule of the Code Napoléon: 'la recherche de la paternité est interdite '—much regretted in the former Kingdom of Westphalia when I was a student at Göttingen—this benevolent rule is reversed when we come to those who, like Robert, have to deal with mythology. Nothing is allowed to stand and, of course, the Sophoklean genealogy of Oidipus is wholly rejected. Laios and Oidipus were foreign intruders. They are neither Labdakidai nor Kadmeians; nor was Laios (which is being interpreted Publius) the father of Oidipus. Publius, by the way, is Wilamowitz's interpretation. To me

Laios seems to belong rather to the stone age, one of the λίθινος γόνος of Deukalion and Pyrrha. Λαοὶ δ' ὀνύμασθεν Pind. O. 9, 50. The names, however, do not matter much when Oidipus ceases to be a chthonic god. The story that he killed his father and married his mother had to be provided for in some way—and the readiest shift was the exposure business familiar to the legends of every country from Moses in the bulrushes down. The Spartans had their Taygetos, as the Thebans their Kithairon. Every child knows of the Babes in the Wood. Every modern policeman is familiar with the front doorstep and the vacant lot. But the Kithairon version has a rival in the boat version. And how common the boat version is we all know, and, if we do not know, there is Usener's Sintfluthsagen to tell us, and, if we accept the further story of Polybus, King of Corinth or Sikyon, as an adoptive father of Oidipus, water carriage makes lighter draughts upon the constructive imagination than land carriage. But either is easier, anything is easier, than the next question, the meeting of father and son, where they met, how they met. This topographical study with ample and fresh illustrations takes up page after page, is complicated with the question whether Laios was in a chariot and Oidipus on foot or both in chariots, and still further complicated by the Delphic influence which sought to contravene the local tradition to the greater glory of the oracular shrine. For me at least a summary is impossible. Why did Oidipus set out? One tradition has it ἐπὶ ζήτησιν ἵππων. The hunt for cattle, for horses, is a familiar *motif*. Saul, the son of Kish, offers a curious parallel, kingdom and all. Or was it ἐπιζητῶν τοὺς γονέας? If so, how were his suspicions aroused? The queer story that Oidipus went to Delphi ἵνα τὰ τροφεῖα ἀποδιδῷ τῷ 'Απόλλωνι is evidently a Delphic fabrication. Everywhere we find traces of tampering with a strictly local legend in which Teiresias was the head centre of prophecy—a thesis of Schneidewin's. The Märchen is there and Robert tells it in a Grimmesque fashion which reminds one of Dr. Arnold's attempt to reproduce in fairy-tale form the legends of Ancient Rome, legends made up in good part by those lying rascals, the Greeks. Then comes the 'Heldenlied' which, after the manner of the 'Heldenlied', takes up a chapter of the story at a time, and the reader's mind wanders off to Robert's Studien zur Ilias and Bethe's new book. But the point is that there is no epic form of the oracle delivered to Oidipus and we have to fall back on the tragic poets and the mythographers. It is this oracle that has given all the trouble. Of the various versions Sophokles has chosen the best. The oracle was a prediction, not a warning, whereas the oracle to Laios was a distinct warning. <But what is an oracle for except to give counsel?> So we

come back to the original fraud of the Delphic priests who must needs take away the glory of the genuine old Boeotian seer, Teiresias, and have woven a tangled web that, according to Robert, not even Sophokles has been able to straighten out, so that the Sophoklean story of Oidipus is full of improbabilities and contradictions—not a perfect chrysolite. The story of Oidipus as reconstructed, from Homer, from Hesiod, from fragments of old epic poetry is far different from the tragic knot tied by the dramatists. The scholia to the Νέκυια 275 go so far as to say ἀγνοεῖ <ὁ ποιητὴς> τὴν τύφλωσιν καὶ τὴν φυγὴν τοῦ Οἰδίποδος. Take away the blinding and the exile and one is tempted to say with the Aristophanic Euripides, ἀπολεῖς μ'. ἰδού σοι· φροῦδά μοι τὰ δράματα. But Robert thinks that the exile is established beyond a doubt and the blinding may well be included among the ἄλγεα left to Oidipus by the Homeric Epikaste.

However that may be, the two figures that change the whole situation are post-epic. By the introduction of Eteokles and Polyneikes, the legend is 'heroized'. Eteokles is an old name without special significance. The name Polyneikes speaks for itself. It is a growth from the story of the war but the story of the War between Argos and Thebes cannot be a mere 'ash-play' of the fancy. It was one of the many assaults that the mighty Kadmeia had to weather in the Myceno-Cretic period of the second half of the second millennium B. C. Several of these wars had been wrought by the Saga into a great collective picture out of which the Thebais arose, not in Greece proper, but in Ionia. The seven gates were seven successive gates, not seven gates in a continuous wall, <a symbol of the way in which the great war between Argos and Thebes took up into itself all the wars the city of Kadmos was involved in through its long history>. Two of the greatest heroes of the Thebais are not Peloponnesians, to begin with, but were drawn into the conflict by matrimonial alliances, Tydeus and Amphiaraos. Originally they were not vassals or allies of Adrastos. Tydeus is reported to have put Ismene to death because of her intrigue with Theoklymenos for which name we are to read 'Periklymenos'. This he did at the bidding of Athene. It is a sheer impossibility, according to Robert, that this should have taken place during the siege of Thebes by the Seven.

The whole story belongs to a different sphere, the struggle between Athene and Poseidon for the mastery. Periklymenos was the son of Poseidon, an enterprising lover like his father before him. Elsewhere, a plucky warrior, he is represented in a vase-painting as fleeing before Tydeus. The seduction of a priestess is a shameful affair. Tydeus, when his time came, was slain by Melanippos who clearly belongs, as his

name shows, to the Poseidon cycle. The conclusion of the whole matter is this : Favorite of Athene, enemy of the sons of Poseidon, Periklymenos and Melanippos, slain by Melanippos before Thebes, either a bastard or begotten of an incestuous union, a criminal stained with the blood of kindred—such was the figure Tydeus made in the imagination of the poets <a figure not effaced by Diomed, his resplendent son>. A companion in arms to Tydeus was Amphiaraos of Oropos, and according to Robert they made a campaign together against Thebes and were afterwards drawn into the story of the Argive expedition. Amphiaraos and Tydeus had the same foe, Periklymenos. N. 9, 24: ὁ δ᾽ Ἀμφιαρεῖ σχίσσεν κεραυνῷ παμβίᾳ | Ζεὺς τὰν βαθύστερνον χθόνα, | κρύψεν δ᾽ ἅμ᾽ ἵπποις | δουρὶ Περικλυμένου πρὶν νῶτα τυπέντα μαχατὰν | θυμὸν αἰσχυνθῆμεν. Just before we read ἑπτὰ γὰρ δαίσαντο πυραὶ νεογυίους φῶτας and this is the verse that Robert cites to shew a divergent tradition from that of the famous and much discussed passage O. 6, 15, ἑπτὰ δ᾽ ἔπειτα πυρᾶν, νεκρῶν τ ε λ ε σ θ έ ν τ ω ν, where τελεσθέντων has been variously explained, variously emended. Robert follows Schroeder without a wink in reading τελεσθεισᾶν a more violent emendation than Van Herwerden's τε δαισθέντων—one of the most ingenious of the many emendations of the Dutch scholar and one which he supported by the δαίσαντο of N. 9, 24. Robert's procedure is hardly justifiable.

So far I have commented on not more than a sixth of Robert's elaborate study and the end of the space allotted to reviews has been reached. Enough, however, has been written to give some notion of the riches and the scope of the work, more than enough to warrant the abandonment of my original plan. Perhaps in a future number some competent reviewer will make good the shortcomings of these pages. All that remains for me to do here is to add one more Pindaric note.

In the first of the 'Beilagen' Robert agrees with Maltens in maintaining as against Studniczka the connexion between the Aigeidai of Thebes and the Aigeidai of Sparta, and, of course, *more Teutonico,* he betters Maltens' instructions. This makes Pindar an Aigeid (P. 5, 76), a member of a noble race, and protects him against the slurs of Freeman and others, who treat him as simply a 'paid peripatetic puffer of princes'. He is really the proud Aigeid, as Wilamowitz justly called him years ago, the man who did not hesitate to call the great Hieron 'friend' (P. 1, 92).

B. L. G.

Aegean Archaeology. An Introduction to the Archaeology of
Prehistoric Greece. Pp. XXI+270. By H. R. HALL.
G. P. Putnam's Sons: New York, 1915.

Several good general books about Crete have appeared in
English in recent years, such as Mosso, Palaces of Crete
and Their Builders; Boyd-Hawes, Crete the Forerunner of
Greece; Baikie, Sea-Kings of Crete; and especially Burrows,
The Discoveries in Crete. The first three are too popular and
the last gives too much attention to Egyptian chronology and
is sadly lacking in illustrations. Hall's new book fills a long-
felt want and is the best account in English of Aegean Arch-
aeology and especially of the excavations in Crete. There are
excellent articles by Evans and Hogarth on Crete and Aegean
Civilization in the last edition of the Encyclopaedia Britan-
nica and on Aegean Religion in Hastings' Dictionary of Reli-
gion and Ethics; but Mr. Hall gives succinctly a general sur-
vey of all the remains of the ancient Aegean (Minoan and
Mycenaean) civilization of prehistoric Greece in the Bronze
Age, not only in the islands but on the mainland. The book
begins with a general introduction explaining the scope of the
work and describes in chapter II the excavations which have
been revealing this great civilization, from the time of Schlie-
mann to that of Evans and other modern investigators. Then
in the following chapters are sketched the results of the exca-
vations, the works in stone and metal, the development of the
beautiful pottery. the architecture of the towns such as Gour-
nia and Pseira, of the houses, of the palaces such as Knossos,
Phaistos, and Hagia Triada, of the fortresses, roads, etc.
Then follows an account of the temples and tombs and a dis-
cussion of the decoration of the buildings with frescoes and
sculpture and a survey of the smaller art. In chapter VIII
the method of writing and the system of weights and measures
are treated at length. In chapter IX costume, armor, weapons
and tools, ships, domestic animals, etc. are the subjects. The
matter of the volume is mainly artistic and cultural and one
important feature is the excellent illustrations (33 plates, 112
figures, and one map) which make the volume of more value
and interest than Burrows. Many of the familiar illustrations,
such as the Tiryns bull fresco, are omitted but the latest finds
at Knossos, Phaistos, Hagia Triada, and even at Tylissos (such
as the Rodinesque bronze statuette of a praying man), Goulas,
and Tiryns are reproduced. We have the recently discovered
frescoes of the hunt, of chariots, and of the boar hunt from
Tiryns; but we miss the beautiful and important fresco from
Knossos representing boy and girl toreadors doing acrobatic
feats over the back of a bull. We miss the beautiful steatite
bull's head from Knossos, of which one sees reproductions in
America at Johns Hopkins, at the Metropolitan Museum in

New York, and elsewhere. We miss Karo's restoration of the Harvesters' Vase. We have on pl. XIX the bronze praying woman in Berlin but unfortunately the wonderful chryselephantine snake goddess in Boston was published too late to be included in the present work.

Another excellence of the book is that it is written by a scholar who is assistant in the Department of Egyptian and Assyrian Antiquities in the British Museum and who is so thoroughly familiar with Egypt and its Aegean connections that he can give the proper perspective to Aegean Archaeology. Mr. Hall has already won a high seat as an authority on matters Minoan and Mycenaean by his Oldest Civilization of Greece and by his recent book Ancient History of the Near East (1913) which should be read to supplement the Aegean Archaeology by all who are interested in the history and ethnology of the early Aegeans.

The book shows a thorough digestion of all the literature of the subject. The only titles I miss are Lichtenberg's excellent little book, Die Aegäische Kultur, Leaf's Troy, and Deonna's interesting monograph on the toilet of Minoan ladies. In general on debated points Mr. Hall takes the right point of view. However the violin-like and other marble island figures (pl. XIV) are scarcely meant to represent the dead (p. 25) but are probably idols. Some will doubt that the Lion Gate at Mycenae was made by Cretans and that Tiryns and Mycenae were actually built by Cretans, though no one will now deny Cretan influence; because in Crete there are no great walls like those of Tiryns and Mycenae.

Mistakes are few. Mycenae was destroyed in 468, not 456 B. C. (p. 9) and parts of the missing pillars from the Treasury of Atreus are in Athens as well as in the British Museum (p. 15). P. 32, read westward for eastward; p. 33 pl. XV, 2 for 3. P. 36, some of the Gournia vases are in America. P. 57, the position of the net in which the bull is caught on the Vaphio cups shows that the top as well as the bottom of the cups is thought of as the ground and that "the ragged clouds of a Cretan sky" are due to Mr. Hall's imagination. There are several bad mistakes in Greek accent such as (p. XI) ’Εφήμερις, (p. 143) βασίλικους, (p. 263) ’Ακροπολεῖς, etc. Misprints occur but not frequently and the last sentence on p. 179 has no proper syntax.

In short Hall's Aegean Archaeology is the best book on the subject in English, though attention should be called to the superior work of Dussaud, Les Civilisations Préhelléniques dans le Bassin de la Mer Egée (2d edition 1914).

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REPORTS.

Der Katalog der varronischen Schriften (1–17). A. Klotz shows that the catalogue of Varro's works (cf. Ritschl, Opusc. III), used by Jerome for comparison with the literary productivity of Origen, included all the works of Varro up to the time when he wrote the introduction to his Imagines; for the seventy hebdomads of books, there mentioned (cf. A. Gell. III 10, 17), give exactly the number of books in the catalogue, if we adhere to the MSS. Ritschl made the number 520 by conjecture. At that time Varro had entered upon his twelfth hebdomad. The catalogue includes the libri rer. rust., which he wrote in his eightieth year (ibid. 1, 1, 1). Hence he could not have written another hundred books in the last decade of his life as Ritschl assumed. Jerome says: vix medium descripsi indicem, which cannot apply to the number of books, needed for comparison; but to the condensation of titles (cf. the collective titles: no. 23, singulares X; no. 4, λογιστορικῶν LXXVI). A neat solution of the first title (XLV libros Antiquitatum) is obtained by adding the four books de gente populi Romani to the XVI lib. rerum divin., and the XXV lib. rer. human. (cf. Aug. de civ. dei 6, 3).

Kyprische und palästinensisch-arabische Flächenmasse zur Zeit der römischen Herrschaft (18–32). O. Viedebantt discusses the ἄρουρα passage of Epiphanius (cf. Symmikta II, p. 200), adds a few conjectures, and determines the relative values of the Cyprian and Palestino-Arabic land measures. Then with the aid of Epiphanius he elucidates a fragment, which Hultsch (cf. Metrolog., p. 599–601) misunderstood. This includes a different system of measurement, which probably some tax gatherer added to the table of measures that he was using. The numerous details supplement and correct Hultsch's Metrologie.

Zur Ueberlieferung der ῥητορικὴ πρὸς 'Αλέξανδρον (33–56). K. Wilke compares the MSS of this work with papyrus II (cf. Hibeh pap. I 114 f. and Berl. Phil. W. 1906, p. 1416 f.), showing that the latter (date 285–250 B. C.), in spite of some flaws, offers a valuable criterion. Wendland's assumption of a revised text in the MSS is proved, as f. i., in the change of ἕως ἡβήσωσι (II) to the Hellenistic ἕως ἥβης; but on the whole

the original text, apart from careless errors, has been pre-
served. Wilke reduces the extent of a lacuna (Spengel,
Fuhr), and, further, presents a collation of class b (the
inferior MSS), which should not be neglected in a future
revision of the text, where Π is not available.

Beiträge zum Text ciceronischer Reden (57–69). K. Busche
supplements and corrects Halm's apparatus cr. to the Sullana.
Halm underestimated V, which has indeed more errors than
T; but they are mostly due to copying, whereas T contains
wilful changes. Where T and V differ in word order, some-
times the one, sometimes the other is in better accord with
usage and the laws of rhythm (cf. Zielinski). B. also offers
a few emendations with interesting arguments: pro Muren.
13, <cumulatarum> deliciarum, for multarum d.; pro Muren.
49, cum spe <sui similium> for cum spe militum; de leg. agr.
II 93, quibus primus <adventus> for qu. pr. annus.

Die Composition des xenophontischen Kynegetikos (70–92).
J. Mehwaldt objects to Pierleoni's following the shorter (A)
version of the proem, as the longer one of class O is evidently
also by the same late rhetorical editor. A similar proem in
Ps.-Galen (Kühn XIV 674) supports Norden's attribution of
the proem to the New Sophistic. The original introduction
begins I 18, which resembles those in the de re equ., the
Memorabilia and the Hipparchicus. An analysis of the ch.
II–XI, shows, on the whole, a sensible exposition. The epi-
logue ch. XII/XIII is anticipated. Chapters VII and VIII
are notes on the foregoing parts, just as we find in ch. VII
and VIII of the Hipparch. (cf. de re equ., ch. IX). The
matter of fact style is similar to that of Simon's treatise on
horses (cf. Rh. M. 51, p. 67–69), which Xen. quotes (De re
equ.), and may be a product of Xenophon's youth (say 402
B. C.). Radermacher's proof of spuriousness, based on style,
is unconvincing, besides there are agreements with Xeno-
phon's later more polished style. Horses were out of place in
this kind of hunting, and the distinction between a φιλόσοφος
and a σοφιστής (ch. XIII 6 f.) is intelligible in this work of the
young Socratic scholar and need not be due to Plato. In
XII, 18 ὧν ἐπεμνήσθην is an interpolation.

Inhaltsangabe und Kapitelüberschrift im antiken Buch
(93–107). H. Mutschmann shows that just as the mechanical
book divisions of Alexandrine scholars suggested the book as
a literary unit, which we see definitely established in Polybius,
so the mechanical device of writing κεφάλαια over the columns
of learned volumina developed into the organic sense divi-
sions, i. e. chapters, as we find on both sides of the Didymus
papyrus (Berl. Kl.-Texte I). Laqueur (Hermes XLIII, p.
220; cf. A. J. P. XXXII 465) mistakes these κεφάλαια for

aufgelöste Kapitulationen. Then followed the collection of such κεφάλαια (συγκεφαλαίωσις), prefixed to each book as a table of contents, which, occurring as early as the Roman empire, or even a century earlier, afforded a convenient source to later compilers.

Die Composition und Quelle von Ciceros I. Buch der Gesetze (108–143). A. Laudien shows by a detailed analysis of de leg. I, enough coherence of thought to forbid the elimination of any part (cf. Schmekel, Die Philos. d. mittl. Stoa, p. 47 ff.). The lack of systematic development is due to Cicero's stringing together passages from a work on ethics, which had a psychological introduction. The plan of the book is contained in the epilogue (57–63), as revealed by a comparison. Cicero himself introduced ch. 6. The source, be it Antiochus or Posidonius, gave an extract from Panaetius. There is a striking agreement with passages of the de Off., which were not considered by Schmekel, who, however, recognized Panaetius as the ultimate source.

Die Scene der Perikeiromene 164–216 (144–153). S. Sudhaus gives the text of the Perik., vv. 164–216, which, if not in every case the ipsa verba Men., may be regarded as substantially correct. The completion of vv. 176–216 was made possible by C. Jensen's collation of the Cairo pap. Various questions are discussed and credit given in particular to Leo's emendations.

Miscellen: Hiller v. Gaertringen (154–156) discusses a Thessalian altar inscription (IV cent.): Διὸς Θαυλίου, and identifies it with Ζεὺς Πολιεύς (cf. Paus. I 24, 4, and Hesych., θαύλια, θαυλωνίδαι, θαῦμος ἢ θαῦλος). Θαύλων 'the killer' may perhaps be connected with Gothic *daups* (Bechtel).—Dessau (156–160) identifies Demetrius, the γραμματικός, whom Plutarch represents as just returned from Britain, in the archonship of Kallistratus, 83/4 A. D. (cf. Pauly-W. IV 2596), with the Demetrius inscribed on the two votive tablets found at York over fifty years ago, the region conquered by Agricola (77–84 A. D.), and draws interesting conclusions. The identification by C. W. King (Arch. Jour. XXXIX 1882, p. 23 f.) has been overlooked.

Fascicle II.

Ephorus. 1. Die Proömien (161–206). R. Laqueur makes the simple form of the proems in Diodorus Iᵇ, II, III, and the subsequent rhetorical proems the basis of a general discussion of the manner in which the historians: Polybius, Dionysius of Hal., Josephus, Eusebius, Livy, etc., treated the individual book, or larger sections of their work, and converges his testimony to show that Diodorus was under the influence of

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Ephorus, the pupil of Isocrates, from book IV on (cf. Diod. XVI, 76, 5). The προγραφαί and προεκθέσεις of Polybius XI, 1 are functionally alike but stylistically different; the latter, woven into the text at the beginning of each new Olympiad, have been preserved in IX, XI and XIV, the προγραφαί, outwardly prefixed to the first six books, are no longer extant. He hopes to reach an agreement with Mutschmann (see above).

Noch einmal die mittelalterliche Ptolemaios-Uebersetzung (207–216). J. L. Heiberg warmly commends the account in Harvard Stud. in Cl. Phil. XXI, p. 75 f. of a Vatican MS (S. XIII–XIV), containing a Latin translation of Ptolemy's Almagest, and adds a supplement to his discussion of the same version in a Florentine MS (cf. A. J. P. XXXV, p. 222). The translator's preface, preserved in the Vatic. MS, throws a flood of light on the MS tradition. The beautiful Greek MS Marc. 313 (s. X), was acquired in Constantinople 1158 B. C. by Aristippus, the ambassador of William I, king of Sicily, to Manuel I Comnenus. A student of medicine in Salerno (anonymous), hearing of this, obtained a Greek copy from Aristippus, and having prepared himself by translating Euclid's optica, etc., then made a Latin version (about 1160 A. D.), which shows improvements that are probably due to Eugenius, a learned Greek mentioned in the preface. This was the source of the above Vat. and Flor. MSS, whereas it can only be said of the Greek MS Marc. 311 (1300 A. D.) that it shows kinship with the above inferred Greek copy of Marc. 313. The opposition of the theologians to the study of astronomy, and to the quadrivium in general, is revealed. Heiberg also considers the later history of the above MSS, and gives extracts from a different Latin version in Dresden (Db 87).

Archäologische Nachlese (217–253). XX (cf. A. J. P. XXIII, 336). C. Robert bases a detailed discussion of the Pergamene altar frieze on the admirable publication of H. Winnefeld, and derives the gigantomachy wholly from Hesiod and Aratus. He identifies Nereus, Doris, etc., and with them Hephaestus; again Dionysus, Silenus (cf. Eur. Cycl. 5 f.), Rhea, etc. Opposite Rhea was Oceanus with a hammer (cf. Etym. Magn. Ἄκμων=Ὠκεανός). Twelve constellations were represented: Orion, Virgo, Ophiuchus, Callisto (the most beautiful figure), etc. He estimates four slabs, at least, for Heracles on the east side.—XXI. The elaborate scene on the sarcophagus slab, imbedded in the wall of the Sala del Meleagro of the Vatican, represents Ostia with its temples of Magna Mater, and Isis, theater, lighthouse, etc., and symbolizes the city in the woman resting on the knee of Ora Maritima. The youth pouring wine represents the portus Augusti, built by Claudius and enlarged by Trajan.

Zur Geographie der unteren Kaikos-Ebene in Kleinasien (254–260). A. Philippson adduces geological reasoning against Dörpfeld's view (Athen. Mittheil. XXXV, p. 395–399), that in Strabo's day the gulf of Elaea lay north of Mt. Cane. Strabo, it is true, places Elaea north of Cane in XIII, 1, 51 (607); but XIII, 622 (not cited by D.) is correct. The confusion in XIII, 581–622, is due to the use of different sources. Strabo also wrongly places Pitane near the river Euenus.

Hippokratische Forschungen (261–285). II De Victu (cf. A. J. P. XXXV, p. 222/3). H. Diels presents textual notes and emendations to περὶ διαίτης I, 1–24, with the aid of a photogr. copy (white on black) of Vindobonensis (Θ), and a copy of P (Paris. lat. 7027), citing for the first two chapters etc. Hermes XLV, p. 138–150; for the rest his Heraclitus (² Berlin, 1909). Although D. had distrusted the Latin translation (cf. A. J. P. XXXV, 223), he found it useful (cf. Heiberg, A. J. P. XXVI, 227). The predominance of unassimilated forms in Θ, like συνγράφειν, shows the older orthography, which occasionally appears in M, though here the usual Byzantine assimilation prevails. Traces of erasures (cf. τὸ θεὸ (=τῷ θεῷ) for τοῖσι θεοῖσι) reveals the work of a Christian corrector; the same zeal appears in P. Occasionally Ionic forms, which were usually displaced by the koine, may be recovered; viz., the rarely preserved ἔπειτεν in ἔπειτεν αμιβεῖ, not altered because read as ἔπειτ᾽ ἐναμείβει. The Latin translation is often indecisive, or incorrect; viz., optime sciens for εὐφρόνη, read as εὖ φρόνει.—III De Flatibus. D. continues with valuable notes, showing the influence of rhetoric, and seeks to recover original forms and words.

Zeus Thaulios (286–291). F. Solmsen connects θαύλιος (see above), with the Lydo-Phrygian name Kandaulas, citing Hipponax's verse: Ἑρμῆ κύναγχα, Μηιονιστὶ Κανδαῦλα (κυν=καν). Hence the priest who officiated at the βουφόνια (cf. Suidas, θαύλων, βουφόνια) preserved in his title Θαύλων (= throttler) a trace of the method of sacrifice practised in the stone age. The θαυλωνίδαι are to be classed with the Κήρυκες, Βουζύγαι, etc.

Miscellen: Fr. Leo (292–295) interprets Plautus Bacchid. v. 107 in the light of Menander and Alexis, suggesting coetu hominum for nescio qui. Plautus closed this first act, only partially preserved, with the entrance of a κῶμος. Verse 108 is a later addition. Fr. Leo (295–296) presents readings for the text of Persius and Juvenal, furnished by E. Schwartz from Montepessulanus 125.—G. Plaumann (296–300) defends his thesis that the cult of Ptolemy I in Ptolemais as Θεὸς Σωτήρ existed as a city cult down to Roman times against W. Otto (cf. A. J. P. XXXV 486/7, 490).—O. Kern (300–303) discusses ἱεροί and ἱεραί (not priests), frequently mentioned in

inscriptions, especially those dealing with mystic and chthonic cults.—O. Kern (304–305) restores inscription no. 8 in Oester. Jahresh. XIII, 1910, p. 45 [καὶ ʼI]άσῳ κτλ. (cf. Paus. V 7, 6; 14, 7; IX 27, 8), thus gaining evidence for the worship of one of the Δάκτυλοι ʼΙδαῖοι at Erythrae.—Ch. Huelsen (305–308) denies the existence of a curia Tifata in ancient Rome (cf. Paulus-Festus, p. 49; 366); instead there were two tifata (= iliceta) : t. Curia and t. Mancina.—D. Detlefsen (309–311) conjectures for Σιγούλωνες, inhabitants of the Cimbric Cherso-nese (cf. Ptolemy 2, 11, 7), Γονίωνες (cf. Pliny 37, 35).—Fr. Leo (311–312) offers emendations to Menander's Samia.—K. Meiser (312–313) now adopts Lundström's καθαιρῶν for καθαί-ρων in Förster's ed. of Libanius I, p. 7 (cf. A. J. P. XXXV, p. 487 end), supported by καθελών in Plut. Moral., p. 734 F.—Ed. Luigi De Stefani (313–315) shows Tzetzes' dependence on Aelian for anecdotes of eagles. Phylarchus is cited only in Ael. hist. anim. VI, 29.—K. Praechter (316–317) rejects Busse's ὄρεξον for βρέξον in David Prolegomena (Comment. in Aristot. XVIII, 2), p. 34, 6 ff., and proposes οἱ περικρατητικοί for οἱ Περιπατητικοί.—K. Praechter (317–318) advocates placing ἐν ʼΑθήναις (with or without brackets) in Marcel. vit. Thuc. 3, after the second occurrence of ἐφʼ οὗ ἄρχοντος. The use of these words as catch-words caused the dittography.—P. Jacobs-thal (318–320) presents an improved text of the epigram Kaibel epigr. graeca e lap. coll. 430, with a commentary by J. Partsch.

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GLOTTA, Volume V. 1913–14.

Pp. 1–8. W. Havers, Zum Gebrauch des Dativs in den italischen Dialekten. The 'dativus sympatheticus' (e. g. *inuk ereçlu umtu, putrespe erus,* 'dann soll er beiden Gottheiten den Altar salben', = 'den Altar beider Gottheiten') and the 'purely adnominal dative' (e. g. *appei arfertur Atiersir pop-lom andersafust,* 'ubi flamen Atiediis populum lustraverit '), in both of which the dative is substituted for the genitive, or rather may vary with it, are rare constructions in the Italic dialects as we know them. But on the basis of the Latin evi-dence we must assume that they were common in primitive Italic ; the restricted sphere of the Oscan and Umbrian monu-ments is responsible for their rarity therein.

Pp. 8–47. K. Witte, Ueber die Kasusausgänge -οιο und -ου, -οισι und -οις, -ῃσι und -ῃς im griechischen Epos. This is another of Witte's searching and illuminating studies in the interrelation of Homeric forms and meter, which have en-

riched every volume of Glotta so far. The entire article should be read and studied in order to be appreciated.—Forms in -ου, instead of older (and in the Epic normal) -οιο, occur *only* (1) when -οιο forms are metrically impossible, or (2) in verses or phrases formed after other rhythmic patterns. 'A third possibility does not exist'. The prosodic values of different forms determine the relative frequency of the two endings. Thus the -οιο forms are prime favorites from choriambic nominatives (−∪∪≍), also from nominatives scanning −−≍, and ∪∪≍, while -ου predominates from nominatives scanning ∪−≍, −≍, and ∪≍. In all these cases, however, the rule holds equally good that the -ου forms are always based on imitation in some way or other. Either (a) verses containing them are modeled upon other, older, verses—even though in a very few cases Witte confesses his present inability to point out the model verse; or (b) the -ου forms are due to the fact that other cases of the same stem regularly occur in particular parts of the hexameter, where forms in -οιο would be difficult or impossible; or (c) they are due to the more general tendency to make all forms of the same paradigm metrically equivalent, so far as possible—a principle of Homeric diction upon which W. lays great stress, and which is only a logical development from (b). (Cf. below, next article.) Generally speaking, the same principles are involved in the use of -οις for -οισι and -ης for -ησι; only that in these cases the younger, or at least popular rather than epic, forms in -οις and -ης are very much rarer than the -ου forms. In fact, a very large majority of the forms traditionally written with endings -οις and -ης occur before initial vowels, and should in Witte's opinion be written -οισ' and -ησ', i. e. -οισι and -ησι with elided final ι. The prosodic construction of words determines here also the relative frequency of the endings; Witte shows that -οις and -ης are commonest, or, as he says, came into use in the epic first, with words whose nominatives scan ∪−−, and are rarest (latest, as he says) with those scanning −∪∪≍.—Witte's general rule, of which the facts in this particular case are only one illustration, is as follows: 'New forms (that is, poetic new creations or forms of the popular language) were introduced into the Greek epic solely because of metrical necessity or on the analogy of particular models'. Again, a still broader formulation: 'The language of the Greek epic is a creature (*Gebilde*) of the epic verse. It (the verse) dictated the linguistic forms. For every Homeric word-form it is possible to determine the reasons why precisely it, and not any differently fashioned or synonymous formation, is used . . . It is our duty to discover these laws'. Compare also the following.

Pp. 48–57.   K. Witte, Zur Frage der Aeolismen bei Homer.

The gradual supplanting of inherited Aeolic forms in the epic diction by neologisms, and particularly by popular Ionicisms, is illustrated by the supplanting of old dative forms in -εσσι by newer forms in simple -σι. These latter are introduced owing to considerations connected with Homeric verse-construction, just as in the cases treated in the preceding article; here, however, the newer, Ionic forms in -σι have gone far towards driving out their rivals. The change is again due largely to the above-mentioned tendency to make forms within the same paradigm metrically equivalent; the ending -εσσι is generally a syllable longer than the corresponding nominative, genitive and accusative forms, while the dative in -σι is not. In general 'where double-forms exist side by side in Homer, those which fit (prosodically, in the manner just illustrated) into a paradigm are very commonly the younger forms'.

Pp. 57–79. W. Aly, Lexicalische Streifzüge.—1. Ἀρέθουσα, 'die Gefällige'.—2. σημάντωρ.—3. θυμέλη, originally 'Tummelplatz' (not 'Räucherplatz').—4. Εὐρώπη, not connected with εὐρυ-οπ-, but with εὐρώς 'moisture, mould' and the like.—5. Φοίνιξ.

Pp. 79–98. W. A. Baehrens, Vermischtes über lateinischen Sprachgebrauch. (Continued from Glotta IV. 266 ff.).—X. Indicativus pro Imperativo. Occurs occasionally in Latin, even without a preceding imperative.—XI. *qui(s) = quidam.*—XII. *omnia = omnino*; oldest example Lucr. II. 456.—XIII. Einiges über die Konjunction *quod. Quod* is used in early and late Latinity in the sense of *quasi*, of consecutive *ut*, of *ut* 'as', and of temporal *cum.*—XIV. Ueber *quoque = que* (oder *autem*). Frequent from the 1st century A. D.—XV. Abwechslung von Superlativ und Positiv.—XVI. Ellipse von *tempus* (with *prius, superius, vetus*).—XVII. *libertas = liberalitas* (as early as Valerius Maximus).—XVIII. *ire*: 'sterben' (poetic and popular).

Pp. 99–170. M. Lambertz, Zur Ausbreitung des Supernomen oder Signum im römischen Reiche: II. (Continued from Glotta IV. 78 ff.). This article enumerates and discusses in great detail the *signa* found in Greco-Roman times in Egypt, Syria and Asia Minor. L. summarizes the results of his investigations as to this method of nomenclature as follows: 'The origin of the Greco-Roman usage of double names is found in a very ancient Egyptian usage (starting from the habit of putting a person under the protection of two gods, by giving him both their names). After the foundation of the Hellenistic empire there appears, first of all in Egypt, a new motive, namely the anxiety of the natives to assimilate themselves even in name to the ruling nationality (so that an Egyptian would assume a second, Greek name).

This anxiety is also responsible for the fact that this style of nomenclature proved convenient and became widespread in other bilingual Hellenistic countries. It became common first in Syria, then in Asia Minor. About the beginning of the empire it spread from the east through Greece and Rome to the western limits of the Roman dominion; as it increased in popularity it lost to a large extent its original significance and became a mere fashion. In the west it acquired from the 2d century A. D. the new function of distinguishing the name commonly used as a person's appellative from the often numerous other parts of his formal and official designation. The custom lasts in the west as long as our inscriptions last; instances are found in post-Gothic times. In the east likewise they are traceable in inscriptions and papyri to the 7th century, and in Byzantine historians to the 10th, about which time we find the *supernomina* or *signa* developing into family-names'. A separate index to the *signa*, etc., recorded by Lambertz is furnished by Ottenjann on pp. 389 ff. of this same volume.

Pp. 170–190. A. Buturas, Ueber den irrationalen Nasal im Griechischen. An attempt to enumerate all the cases of the insertion of historically unjustified nasals in Modern Greek (both literary language and dialects). The author's thesis is that the phenomenon is a 'spontaneous development', meaning that no single law, or small group of laws, will account for all the cases.

Pp. 191–2. O. Probst, *Acrudus.* Cf. Thes. Ling. Lat. s. v. The sole occurrence of the word, *acrudo,* is a corruption of *acra nuda.*

Pp. 193–7. E. Schwyzer, Zur griechischen Laut- und Wortbildungslehre. 1. ἔρνος (beside ἔρνος).—2. κρόμμυον.—3. ἄδεια, ἔκδεια, ἔνδεια.—4. γεννᾶν.—5. περσύας; for *περυσύας, for *περισύας or *περυσίας.—6. ἑκατόν; the disappearance of the ν of *ἐν-κατόν is due to dissimilation.—7. λεβηρίς: τὸ λέπος τοῦ κυάμου *exuviae, pellis deposita.* (Cf. τριετηρίς.)—8. ὀιζύς; for ὀ-ϝι-ζύς (root *sed*).

Pp. 197–202. G. Pasquali, Οἰκιστήρ. This is a formation properly foreign to Ionic-Attic. It was an epithet of the founder of Cyrene (Pindar, Hdt., Callimachus).

Pp. 202–208. J. H. Schmalz. Satzbau und Negationen bei Arnobius. A few miscellaneous peculiarities of construction, in part consisting of archaisms.

P. 209. J. H. Schmalz, Synesis oder Schreibfehler? '*Prolatio cupidinis atque irae ... fuerant ...*' (Arnobius); not to be emended to *fuerat;* construction according to sense.

Pp. 209–214. J. H. Schmalz, Mischkonstruktionen im Lateinischen. Some miscellaneous but individually interesting

late-Latin blend-idioms; e. g. *quanti valere,* a blend of *quantum valere* with *quanti esse.*

Pp. 214–221. J. Compernass, Vulgaria.—1. *Quam* with the comparative instead of *quam* with the superlative.—2. *Ictus oculi* and *instans = momentum (temporis).*—3. *Consultare* and *consulere.*—4. *Fui* 'ich bin gegangen oder gekommen'.— 5. *Magis* had come to be a simple adversative conjunction (French *mais*) even in the 5th century, possibly in the 4th.— 6. *Sin autem, sin alias* 'otherwise' (also in Classical Latin).— 7. *Aut non* for *annon* or *necne,* in double questions.—8. *Vel* 'at least' (just in the same way Vulgar Greek κἄν is used).

Pp. 221–237. E. Lattes, Etrusca. I. Vi ebbero in etrusco verbi in *-sa* e nomi in *-s* plurali? New arguments in defense of L.'s affirmative answer to this question, especially in reply to Torp's dissenting opinion.—II. L'accusativo sg. etrusco usci forse in *-m* o *-n*? Answers in the affirmative.—III. Etr. *suθi* e *lena, -al -ale -aia, aisna hinθu.* Replies to criticisms directed by Herbig, Glotta IV. p. 176 ff., against the author's previously exprest views on these words and suffixes.—IV. Ancora di alcune voci etrusche in *-m* o *-n*. Cf. Glotta IV. 224 ff.

Pp. 237–249. G. Herbig, Die faliskische Kasusendung *-oi.* 1. Dative auf *-oi.* Enumeration of examples, which seem to make this function of the ending certain.—2. Weibliche Nominative auf *-oi, -o,* griech. *-ωι, -ω.* Apropos of CIE. 8036 ff., containing the forms *titoi* (and *tito*) *mercui,* which H. believes are nominatives feminine, Latin *Tito(i)*Merconia.* He would bring such forms as *Tito(i)* into relation with the Greek nominatives feminine in *-ω(-ωι),* that is, old *-ôi* stems. He enumerates traces of the same endings in Latin inscriptions.

Pp. 249-253. G. Herbig, Zur Mouillierung des *l* im Vulgär-lateinischen. Contains a small list of inscriptions, from widely different times and places, showing (in H.'s opinion) palatalization of Latin *l.* Perhaps clearest and most interesting is the form *piacet* for *placet,* in an inscription of 101 A. D. from Perrhaebia in Thessaly. H. draws no definite conclusions; he says his materials indicate that the phenomenon arose sporadically at different times and places, sometimes under the influence of pre-Latin local languages.

P. 253. G. Herbig, Zu Glotta IV. 168 ff. *Tresivio* should be read for *Trevisio* in the article referred to.

Pp. 253–8. F. Sommer, Der italische Pronominalstamm *eo-.* In Oscan-Umbrian this stem is found only in the nom.-acc. cases, which suggests that the paradigm probably started from one of these forms; this rules out Brugmann's otherwise highly improbable derivation of it from an instrumental singular feminine **eįâ.* Sommer would start with the form

ei-om=Skt. *ayam*, an old nominative singular masculine, consisting of *ei*+ *-om*, the latter element being analogically taken from the ending of *eĝhom*, just like the ending of Sanskrit *tu-am* (*tvam*), *id-am* etc., Oscan *siom*, *tiom*, the last of which is actually used as nom. as well as accus. The form *eiom* (Lat. *eum*, Oscan *ion-c*) came to be used first optionally, and then exclusively, as an accusative, because of its accidental resemblance to other accus. forms in *-om*. After this had happened it was natural to form analogically an accus. fem. *eiâm* (*eam*), and finally in Latin all the other cases of the stem *eo-*. Sommer might have mentioned in this connexion, it would seem, the quite analogous Skt. accus. fem. *imâm* and the plurals *ime*, *imâs*, *imâni* etc., patterned after *imam* (i. e. *im+am*, IE. *im-om*), on the analogy of *tâm*, *te*, *tâs*, *tâni* etc.: *tam*, and the like. The writer agrees with Sommer in regarding Skt. *imam* as IE.*im-om*, not *-em* (as Brugmann, Johannes Schmidt and others have maintained). Sommer's explanation of *eo-* seems at least the most plausible one which has yet been advanced.

Pp. 259–368. Literaturbericht für das Jahr 1911. Greek, by Kretschmer; Italic Dialects and Latin Grammar, by F. Hartmann; Syntax, by W. Kroll.

P. 368. G. H(erbig). Zu Glotta V. 252. An additional note.

Pp. 369–398. Indices, by H. Ottenjann.

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BRIEF MENTION.

For a *commentarius perpetuus* on *Aischylos*, says WILAMO-
WITZ in his recent edition of the poet (Weidmann), the rest of
my life would not suffice, and most assuredly the brief residue
of my days would not suffice to utter the thoughts that arise
in me as I read the commentary he has written and the volume
of interpretations that accompanies it. Thoughts are they that
wander into the vastness of the great scholar's achievements,
that circle about the long series of my own adventures and
misadventures in the society of masterpieces, to allude once
more to the famous remark of Anatole France. My first
Aischylos—I have it still—was a Carl Tauchnitz text of 1829,
a pocket edition, neatly printed on strong writing paper—
'Ladenpreis 6 groschen'. The word 'groschen' gives me
pause. In my student days it was 'groschen' not 'marks'.
There is a world of meaning in the change of currency, and
late in the eighties, when I found at one of my old haunts on
the Rhine that 'zehn Groschen' still held its own by the side
of 'eine Mark', my youth came back to me as with a flood.
The multiplicity of German coinage which was once a nuisance
in practice became in memory a matter of romance. Nothing,
in fact, is more significant than coinage. In my boyhood the
United States mint was not equal to the demand and Mexican
coins were current so that when I was at Gytheion, the port of
Sparta, I was able to recognize at once a coin that puzzled a
German scholar and a local antiquarian. Even Jacobuses
were occasionally seen, and these exotic coins showed forth a
certain comradeship in the settlement of the New World. So
long as the different states used familiarly different reckonings
in shillings and pence, we were colonials. Our standard was
the standard of the old country. Far into the fifties Virginia
shopkeepers carried their accounts in both standards, eighteen
pence in one column, twenty-five cents in another, and we
South Carolinians were proud that our seven pence was as
good as the York shilling. But what has all this to do with
Aischylos? Much every way. It might serve, for instance, as
an excursus to the passage in the Agamemnon that shows
Aischylos to have been as alive to debasement of the coinage
as was Aristophanes.

$$\text{κακοῦ δὲ χαλκοῦ τρόπον}$$
$$\text{τρίβῳ τε καὶ προσβολαῖς}$$
$$\text{μελαμπαγὴς πέλει.}$$

But, what is more to my purpose, it offers a twentieth century solution of the tortoise riddle which WILAMOWITZ shirks. It is perhaps the absurdest of the various absurdities that I have in store to match the other interpretations. The eagle that let the tortoise fall on the bald pate of the great dramatist is the accepted symbol of the poet and the poet's art. The tortoise is the symbol of hard cash and as such figures on the coins of Aigina, the great merchant island. The poet lost money in his venture. The drama had ceased to pay. Epinikian poetry, the poetry of Pindar, was the only remunerative line, and Aischylos was a commercial failure, 'dead broke' when he died. It was out of consideration for the feelings of his family that the men of Gela made no mention of his achievements as a dramatist—a point often noticed, never before properly interpreted.

To come back to my Tauchnitz Aischylos. Like all men of my time, I own a number of these old Tauchnitz editions; and some of them are a joy to me, notably the Aristophanes, by reason of their faulty texts, showing as they do the advance of textual criticism just as the old Variorum editions give evidence of the progress achieved in exegesis. Both may be made to serve as adminicles to the work of the Greek Seminary. The veriest novice can be taught by these old Tauchnitz editions to restore the readings of the best MSS., to correct the false spellings, the bad forms, the abnormal syntax—an encouraging exercise in the art of handling texts. And, if the Tauchnitz Aischylos of 1829 carries me back to the old days, the Weidmann Aischylos of 1913 performs the same office in a different way. WILAMOWITZ, it seems, has a cult for the heroes of the philological past, such as the once overrated and then underrated Heyne, whose reputation suffered such scath in the great Wolfian controversy. Some of these heroes belonged to times within my remembrance. WILAMOWITZ's *Sappho u. Simonides* was dedicated to the memory of Welcker, and this Aischylos of his is dedicated to the memory of Gottfried Hermann. It is better thus. The history of friendships among scholars does not warrant dedications to living men, and one recalls grimly how Boeckh dedicated one of his early works to Gottfried Hermann. Boeckh I knew and have dared to call him my master. Perhaps I have insisted too much on the fact that as a lad of nineteen I sat at his feet for one semester and contemplated his shoe-latchets. But Hermann was dead before I went to Germany, died in the great year 1848, the beginning of an era. His personality was still felt in my time, his power is still felt, as WILAMOWITZ testifies. In

my meagre library of those far-off days, Hermann had a large place with his Elementa doctrinae metricae, his edition of Sophokles, his bulky Ad Vigerum. When his posthumous Aischylos came out, I possessed myself of it at once as I had possessed myself of Lachmann's Lucretius. Such books were epochs in the life of a young scholar. At a later day I have known one man to date by Wilamowitz's Analecta, more to date by the Herakles. Wonders were expected of Hermann's Aischylos. There were, it is true, disillusionments, but at that period I had no personal right to disillusionment and my wrath was kindled by unfavorable comments, especially by those that emanated from English sources, to which at that time of my life my attitude was distinctly hostile, as is abundantly shown by my maiden review article, The Necessity of the Classics, published in 1854. Our German teachers had given us to understand that with the exception of the greatest Britons, such as Bentley and Porson and Dobree—a Channel Islander, by the way—English scholars were a lot of amateurs; and I can recall Boeckh's sarcastic comments on current English performances. Of Paley's editions, for instance, he spoke with a certain degree of allowance, as possibly useful for the youngsters of Oxford (Oxfort) and Cambridge (Kämmpritsch) and I am afraid that I have never overcome my prejudice against Paley (Pallai) in spite of my sincere admiration of the sacrifices he made for his faith. Yes. The English were amateurs all, and in a recent memorable article on German 'Kultur' Gilbert Murray has confessed judgment.

Imagine, then, my indignation when I saw that one George Burges had dared to append to the Bohn translation of Aischylos by Buckley, a series of animadversions on the emendations proposed by Hermann—Burges, of whom my beloved teacher, Schneidewin had written in the preface to his Babrius: Per rabiem quid effici possit, non sivit inexpertum G. Burgessius (*sic*) cuius θυρσομανεῖς bacchationes exhibent Acta Phil. Parisina. That did for Burges in my youthful eyes. 'Fort mit Schaden'. And I look with amusement at the pencilled notes with which I accompanied Burges's criticisms. For λέξεται Ag. 160: οὐδὲ λέξεται πρὶν ὤν Hermann proposed οὐ λελέξεται, whereupon Burges remarked: 'However, he neglects to shew that λελέξεται is used as a fut. pass. as well as λέξεται'. As the passive use is the prevalent use for the tense that our grandfathers called the paulo post futurum, this is a grammatical 'howler' of the first order, or if you choose, of the loudest bray, and I wrote on the margin, Plato, Rpb. 5, 457 B: κάλλιστα γὰρ δὴ τοῦτο καὶ λέγεται καὶ λελέξεται ὅτι τὸ μὲν ὠφέλιμον

καλόν, τὸ δὲ βλαβερὸν αἰσχρόν. To be sure, Wɪʟᴀᴍᴏᴡɪᴛᴢ does not accept λελέξεται and refuses to consider Hesychios' gloss: λελέξεται· λεχθήσεται, as a reference to this passage, but surely we can dispense with Hesychios in a matter of this sort. λελέξεται is old fashioned for εἰρήσεται, the most common example of the future perfect, and the sense seems perfect, 'will have no abiding mention'. It might be worth while to compare Burges's views at other points with those of Wɪʟᴀᴍᴏᴡɪᴛᴢ, but I cannot take it upon me to decide whether the verse of Aischylos will apply to him : οὐ λελέξεται πρὶν ὤν.

Now while it is true that 'howlers' have all nations for their own, still grammatical 'howlers' are among the pet sins of the English, whereas metrical 'howlers' seem to flourish especially on German soil—a failing emphasized by Gilbert Murray in the article to which I have just referred, a failing generally attributed by English scholars, among them the aforesaid Paley, to the neglect of practice in versemaking. Of both sorts the Journal has recorded instances not a few— but as this *Brief Mention* is written far from the checks and balances of the back volumes of the Aᴍᴇʀɪᴄᴀɴ Jᴏᴜʀɴᴀʟ ᴏғ Pʜɪʟᴏʟᴏɢʏ, the reader will be spared what a critic has called my ugly parentheses.

Apropos of versification I have elsewhere recorded my boyish enthusiasm for the lyric measures of the Greek tragic poets, first roused by Franz's recitation of the opening chorus of the Septem so that Wɪʟᴀᴍᴏᴡɪᴛᴢ's treatment of the metres of Aischylos wakes a long series of memories from the time when I studied metres under Von Leutsch in Göttingen, and Leopold Schmidt in Bonn, through the years when in my work as a teacher, I availed myself of the metrical schemes of Dindorf, through the years when I was led away captive by the Rossbach-Westphal-Schmidt theories down to the unhappy present when I am distracted by all the new doctrines and cast down by the revival of all the metrical authorities of later antiquity, authorities I had once fancied dead and buried. And yet, though I am under the ban of both metrical schools, I might say all metrical schools, I cannot refrain from trying to find something in Greek metres that appeals to my sense of artistic fitness. Syntax means nothing to me unless it means everything, and metre without vibration is naught. Arabic music, I have read, sets the European's teeth on edge. Greek music, such of it as survives, makes a direct appeal to men of Aryan stock, and even when the music is lost,

the verse to which the music was wedded must retain some traces of the original union. It was a blow to me when years ago in his Commentariolum Metricum WILAMOWITZ denied all ἦθος to the logaoedic—I beg pardon—Glyconic measures, and I have expressed my gratitude at finding that even in the return to the system of the ancient metricians, one who has been immeshed, if not suckled, in a creed outworn may be made somewhat less forlorn by occasional glimpses of agreement with the ethical results of Professor White's analyses. Of course, under this head no great comfort was to be expected from WILAMOWITZ's *Aischylos*. 'Quod eas sum secutus rationes', he says, 'quas duce Hephaestione et diuturno carminum Graecorum (nec Graecorum tantum) usu veras esse mihi persuasi, ne ei quidem mirabuntur, quibus alias rationes aut musica ars aut arithmetica revelavit. Spero autem eis satisfactum esse qui et veterum grammaticorum et mea scripta norunt'. As in my case both conditions fail, there is nothing left for me but the blackness of darkness and I contemplate the summaries appended to the choruses with profound melancholy. Here is one of the sequences. Dochmiac, spondee, cretic, molossus, paeon, iambi, bacchiac, a jumble, as I once irreverently called it, with no central soul. The names are only too familiar and some of the traditional designations are, I grant, convenient for metrical groups, but some of them I positively loathe. To call a verse that burning and sighing Sappho used a Greater Asklepiadean is an insulting anachronism, and to apply the name ithyphallicus to an Aeschylean verse is a profanation. The trochees of Aischylos are laden with heavy thought. The ithyphallicus suggests the Aristophanic gentleman, who threatens Iris with a triple visitation. Ah weel!, as Jebb would have said, he who succumbed to the fascinations of Heinrich Schmidt as I had succumbed before him. The wonderful parodos beginning θρεῦμαι φοβερὰ μεγάλ' ἄχη is called by WILAMOWITZ ἀνομοιόστροφος and that is just the impression it made upon me sixty-five years ago in the stuffy auditorium of the University of Berlin.

Looking over the critical apparatus of WILAMOWITZ's edition I was struck with the frequency with which the names of the earlier editors and critics recur, but I have long since learned to distrust impressions and so I had recourse to statistics, a method much abused of late but always useful for correcting rash generalizations. As a test, I took the Agamemnon, which for various reasons has an especial interest for me. But I soon found that I was in for a more laborious job than I had expected, and that the game was not worth the candle. In

order to make the statistics instructive from my point of view, it would have been necessary to get the percentage of the successes of this and that critic, but that would have involved the counting of all the contributions of sixty odd scholars to the criticism of Aischylos, a large proportion of them being sporadic, as for instance Seidler's ἄπολις for ἀπόπολις (1411) due to his study of the dochmiac verse. A computation like that was utterly impossible in the absence of a complete apparatus, such as WILAMOWITZ does not pretend to give. And then who is to be the judge of the successes? To accept WILAMOWITZ's judgment in all the cases that come up would be an abject surrender, and whilst I have an unfeigned admiration of the world's foremost Hellenist, I am not ready to swing the censer quite so vigorously as Robert has recently done in his Oidipus. Even in the assignment of the corrections to the different scholars I find WILAMOWITZ at odds with Dindorf, the only authority accessible in my present surroundings; and I am tempted to throw my statistics overboard, especially as I have not the statistical temperament. Still some younger man and better accountant may think it worth while to add his 'correxi' to the figures I am able to offer the readers of *Brief Mention*. There are, as I have said, sixty odd contributors, nearly all of the highest rank, to the textual criticism of the Agamemnon, as reported by WILAMOWITZ, and my impression has been confirmed, and that amply. Leaving out, as well we may under the circumstances, Hermann and WILAMOWITZ, the one with some 20 emendations, the other with 34, the old boys have the preëminence; and whilst the ancient saying 'tarde venientibus ossa' is by no means true, still Amatus with his twenty-six successes, counting two or three that Dindorf ascribes to other scholars, beats all comers except WILAMOWITZ himself, and yet in his Preface WILAMOWITZ has intimated that Hermann overrated Amatus 'splendore paucarum emendationum delectatus'. To take them alphabetically Ahrens has 10, Bloomfield 13, Pauw 10, Porson 16, Scaliger 11, Schütz 13, Stanley 16, whereas other editors fare badly. Conington is credited with but two, Dindorf with three, Enger with four, Hartung with three, Franz, to me Franz of blessed memory, with one, one but a lion 948: εἱματοφθορεῖν for σωματοφθορεῖν. Wecklein, of whom Murray has recently made a butt by reason of his metrical lapses has but one to his credit, Weil, the admirable, has seven. Verrall figures here and there in the other plays, but none of his plastic restorations of the Agamemnon has found favour in the eyes of WILAMOWITZ. In one of the two emendations of Conington's accepted by WILAMOWITZ's λέοντος ἶνιν for λέοντα σίνιν (717) I miss the circumflex accent. Headlam, whose Greek poems have been highly extolled by WILAMOWITZ is represented

by three hits, one where he introduces, plausibly enough, that maid of all work γ(ε) into the sanctuary of the text (1607), another where he changes ἔρδειν to ἔρξειν (933) unnecessarily, for ηὖξω is a verb of will, and a third (996) where he substitutes κυκώμενον for κυκλούμενον—a seductive conjecture, for one recalls Archilochos' familiar θυμέ, θύμ' ἀμηχάνοισι κήδεσιν κυκώμενε, but δίνας seems to protect the text. WILAMOWITZ's own corrections of the text and his occasional remarks on Greek usage would furnish material for extended comment, but this is *Brief Mention* and not a review.

In his Preface WILAMOWITZ has told us how his point of view has shifted since he edited the Agamemnon in 1885. 'Pleraque', he says, 'mutabam nulla alia de causa quam quia sermonis Graeci et artis Aeschyleae idoneam peritiam non acquisivi'. Ten years afterwards, on his Choephori, he had attained his full stature and in this edition he has remained true to his principles despite the 'christianissimus livor' of Blass. A tempting theme, this study of the development of WILAMOWITZ whom one thinks of as a Herakles from the beginning. No question of his strangle hold—μάρψαις ἀφύκτοις χερσὶν ἑαῖς ὄφιας. But not to find myself within his reach, I will dismiss the subject, fascinating as it is, with a few remarks on the general subject of conjectural emendation. The tenor will not be unfamiliar to the readers of the Journal, ἀλλ' ὅμως. As an American of the Americans, I can well understand why so few of my countrymen have ventured on speculation that promises so little result, but apart from this consideration, I have long held it to be little short of a crime to advance mere guesses— in the vague hope that some one will adopt them and stand up for them. In the list of WILAMOWITZ's various readings it has happened a couple of times that emendations abandoned by their authors have found a benevolent patron in him, but that is not to be counted on. Now if one examines Amatus's twenty odd corrections, it will appear that most of them are inevitable, most of them have been taken up into the text. They are all simple, and it is these simple changes that hold their own, simple changes that fall within the range that Kenyon prescribes. The 'splendor' of which WILAMOWITZ writes would not shine in a printer's office to-day. They belong to the realm with which some of us are sadly familiar, the realm of proofreading in which every editor performs feats that would be loudly acclaimed, if the language were Greek or Latin and not the native tongue. Haplography and dittography are no mysteries in practice to some of the confraternity who do not even know these convenient technical terms and who lay no

claim to the *divinum ingenium* ascribed, for instance, to Reiske, for whom indeed every classical scholar entertains the highest esteem. What is the glory of discovering a turned type? What of discovering a *caret*, the very symbol of which ∧ stands for the Greek word λείπει? Who has not found occasion to put asunder what the printer has joined together, and join together what the printer has put asunder. It is a matter of context whether one reads 'this creed' or 'this screed'. But, as I write, the ghosts of dead and buried typographical errors, ghosts, which a flirt of the pen might have laid forever, begin to squeak and gibber in the halls of my memory, 'indefensible' for 'indefeasible,' 'row' for 'vow,' 'fornication' for 'formication,' 'Pythagoras' for 'Protagoras,' 'coöperation' for 'coöptation,' 'chronicle' for 'coracle' and hosts of others, some of them signalized in the various Errata of the Journal, the correction of which would make the reputation of any Greek or Latin scholar. At the same time it must be remembered that there are such things as happy mistakes, and a number of these 'felices errores' are recorded by Wilamowitz in his commentary. But neither do these lack parallels. In Malherbe's famous 'Rose, elle a vécu ce que vivent les roses.' 'Rose, elle' was originally a printer's error for the author's 'Rosette,' and to my mind one of the best things in all Herbert Spencer is the story that in the sentimental outgiving 'Pour connaître l'amour, il faut sortir de soi' the idealistic 'de soi' became in type the realistic 'le soir.' Among the triumphs of conjectural criticisms is the fishing out of proper names from common nouns and I have no doubt that my old teacher Von Leutsch was duly proud of correcting the text in Justin Martyr Apol. II, s. f. where he wrote Ἀρχεστρατείοις for ὀρχηστικοῖς, a reading that I accepted gladly and defended strenuously when it was attacked by Bücheler. But as I was stringing together these holiday cowries, my evening newspaper brought me an article in which I read 'Porthos, the author and Aramis' and I asked myself what glory any proofreader would take to himself for recognizing in 'the author' the name of the second of the 'Trois Mousquetaires.' Greek is not so familiar as English after all, and the 'splendid' successes of our elder brethren give rise to furious thoughts of our inferiority, which, if true, comes largely from the fact that we do not steep ourselves in reading as did the scholars of the old time. They followed their noses. We count. But in textual criticism the nose beats the abacus. I have just laid down a book on which I have made two 'splendid' emendations, 'soil' for 'soul' and 'rose like an exhalation' for 'arose like an exhilaration'—though I am suspicious of a joke in the second specimen of my 'divinum ingenium.' When Weil died, one of his eulogists complimented him on a happy emendation

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in the text of a French classic, and what monstrous per-
formances are possible in modern languages has been shewn
by some of the German emendations of Shakespeare's text—
and not German emendations only.   The whole region is full
of slime pits.

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The renascence of Greek studies among the Italians is
claimed, and with some justice, claimed by the Germans; and
this is one of the counts in the furious Teutonic indictment of
those who are more concerned about 'Italia irredenta,' than
about the new birth of classical philology in Italy.   Like their
teachers, the Italians have a mania for bibliography, like the
Germans, they are much given to the discussion of æsthetic
questions.   Such a question is the one treated in a little book by
FRANCESCO GUGLIELMINO, *Arte e artificio nel dramma greco*
(Catania, Battiato, 1912).   A sequel was promised and I have
been waiting for that promised sequel in order to bestow a
Brief Mention upon a treatise which deals with a subject of
unfailing interest to every Greek scholar.   But the sequel has
not appeared or has not reached me, and I am moved to the
present inadequate notice by the last number of the *Harvard
Classical Studies, Vol. XXVI,* which contains an article on the
so-called Unity of Time in Aristophanes by Dr. OTIS JOHNSON
TODD: *Quo modo Aristophanes rem temporalem in fabulis suis
tractaverit.*   In that paper the author reaches the interesting
conclusion that in six, or haply seven, of his plays Aristophanes
has observed the unity of time, whereas in the remainder, to
wit, the earliest plays, he has extended the action beyond the
limits of a single day.   This advance of the more mature
artist in the direction of verisimilitude is quite in line with
that part of GUGLIELMINO's book which deals with the
methods by which the Greek dramatists strove to impress their
audiences with the semblance of verity in their versions of the
old stories.   The artifices—Dr. TODD uses the same word as
does GUGLIELMINO—which Aristophanes employed became
what might be called 'tricks of the trade' in the New Comedy.
We are thus furnished with an illustration of the advance of
technique, the advance of consciousness in art.   Now this con-
sciousness in art is a bugbear to a certain school of Hellenists.
Consciousness, it is maintained, is fatal to the highest art, nay
is fatal to the highest thinking as well.   All the great thoughts
were thought before the advent of Sokrates, all the poetry
that came after the first flowering was infected with the germ
of consciousness.   That admirable scholar, Fraccaroli, has held
forth on the subject in his edition of Pindar and Bodrero has
made it the theme of perfervid eloquence in his Eraclito and
Livingstone has preached the same doctrine in his book on the

Greek Genius. Of course, this view narrows the range of what is genuinely Hellenic. Euripides is excluded. He is a sophist of the sophists. Sophokles is tarred with the same stick, or to be Scriptural and classical, is defiled with the same pitch; his colours are aniline colours. The Kreon of Sophokles has lately been pronounced an orator of the Sophistic pattern, and Aischylos himself, according to WILAMOWITZ, shews in his Prometheus the influence of the early Sicilian rhetoricians, and perhaps it was not so silly in me, as Jebb intimated it was, to see in the Pindaric λόγιοι prose rivals to the poetic encomiasts of whom Pindar was one (P. 1, 94), even if we have to reject Verrall's sportive interpretation of κόρακες ὡς (O. 2, 96), as an allusion to Korax and Teisias. True, the study of Greek rhetoric, and the analytical processes demanded by syntactical research may breed, or seem to breed, a habit of mind unfavorable to the immediate enjoyment of unconscious art, a habit possibly fatal to claims of superior sensibility; and I freely confess that no matter how far back I go, I still find evidences of a reflective spirit. If Homer is court poetry, Bréal is not so far wrong after all. The Greek rhetoricians used Homer as a text-book, or if you choose, as 'timber' for tropes and figures, for types of eloquence. That was going too far perhaps but there is artifice as well as art in the great epics. The word 'rhetorical' has been used by editors of Homer in respect to certain books and those not the least beautiful, although the difference between art and artifice is not so evidently localized throughout as to enable us to distinguish everywhere in the body of Iliad and Odyssey the true poet and the more or less skilful redactor.

But in all fairness I must hie back to GUGLIELMINO. The Introduction gives us the scope of study. While granting the force of Fraccaroli's demonstration of the liberties the Greek dramatists have taken in the choruses of their plays, their anachronisms, their incongruities, still reality has its rights, verisimilitude its claims and these rights and these claims have led to the employment by the poets of certain devices to satisfy their audiences, although when the author says that these audiences consisted of a 'moltitudine in gran parte incolta' he weakens his own argument. There is evidence enough that the Athenian audiences were as keen-witted as any modern audience, if not more so. The book has two parts. The first part has to do with the conventions of the stage and verisimilitude generally, the second part with calculated effect (la ricerca dell' effetto). The first chapter of the first part deals with the devices employed

to save verisimilitude in the use of the chorus after the chorus had ceased to take part in the action and had become a 'perpetuum tragicae artis impedimentum', as the author quotes from WILAMOWITZ. The second chapter treats of the necessity of setting forth for the benefit of the spectators the events preliminary to the action of the play and of the devices requisite for explaining away the resulting inconsistencies, of which, by the way, modern playwrights make small account and which our modern audiences would hardly notice, though they would resent being called a 'moltitudine incolta'. The third chapter tells of the effect that the small number of actors has on the architecture of the drama—a part which I myself had occasion to illustrate many years ago in comparing Macbeth with the Agamemnon. The recent assaults upon the limitation to three actors came too late to be considered by GUGLIELMINO, to his profound regret. The second part deals in the first chapter with the flattering appeals made by the tragic poets to the patriotic sentiments of the public, in the second with the customary and favoured methods of rousing emotion by pathetic speeches ($\dot{\rho}\eta\sigma\epsilon\iota\varsigma$). The third chapter treats of 'i doppi sensi' or amphibology, which, to be popular, postulates a quick-witted public, the fourth of the effect of contrasts level to lower intelligences.

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The bibliography comprises 128 numbers—commentaries and works of general reference are not included. The acknowledgment of indebtedness seems fairly honest, a thing that cannot be said of all bibliographies. It is a tumultuous affair, this bibliography. There is no alphabetical or chronological order, no grouping even as to authors. The vast majority of treatises are by Germans, some 80 per cent. The list reminds one of the constitution of the German army—forceful officers and well-drilled soldiers. In the long array there are not a few shining names. The rest may be described in Aristophanic terms as $\overset{\text{\textasciimacron}}{a}\pi a\xi \pi\rho\sigma\sigma\sigma\upsilon\rho\dot{\eta}\sigma\alpha\nu\tau\epsilon\varsigma \tau\tilde{\eta} \phi\iota\lambda\sigma\lambda\sigma\gamma\dot{\iota}a$. They are not drones, it is true, but they have the fate of the drone who overtakes the queen bee in her nuptial flight. The immense preponderance of German scholarship is not to be gainsaid. Will it survive the shock of arms, not materially but spiritually? Just now German scholars point with pride to their preeminence in Greek studies as a vindication of their 'Kultur', as a satisfactory answer to the charges brought against the Empire for its ruthless methods of warfare. Is the world to be made over after the German pattern? In the years of my Teutonomania we American students used to join our German fellow-students in singing 'Schleswig-Holstein,

meerumschlungen' but there was yet another song, 'Wir wollen keine Dänen sein, Wir wollen Deutsche bleiben'. Change the names and there is a lesson for our times. Not long ago I came upon a letter addressed to me by an American classical scholar, too soon lost to the world of scholars, a letter in which he protested against the title AMERICAN JOURNAL OF PHILOLOGY. The Journal was then a programme and nothing more. He objected strenuously to the provincialism of 'American'. Why not emulate 'Hermes', 'Athena', 'Hermathena'? If he had paid the price for his nationality that I had paid for mine, he would have understood my persistence. What GUGLIELMINO would say to all this now is a matter of curious surmise. To return to his Bibliography, the Italians come next to the 'Praeceptores Italiae'. Few Frenchmen are mentioned except the inevitables, such as Patin, Croiset, Girard. Of the English Verrall is mentioned a couple of times. The name is not always correctly spelt, but for that matter one reads in GUGLIELMINO 'Incidit in Scyllam cupiens vitare Caribdim'—a mere typographical error, it is true, but there are strange lapses in these pupils of the Germans, as I have had occasion to note.

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G. L. H.: Professor CHARLES CHRISTOPHER MIEROW of Princeton University, who, seven years ago, published the first and, until the present, only English translation of the *Getica* of Jordanes, offers it now in a revised form with the additions of an introduction and commentary (Princeton University Press, 1915). He naturally followed Mommsen's text in his translation, and he has met and overcome with dexterity the difficulties incidental to this Low Latin work, with its broken-down syntax and its uncouth vocabulary. Mommsen's introduction and notes, supplemented with a few more recent articles, furnish the material for his illustrative matter. As the original work is distinguished by its lack of literary form, its translator should have informed his uninitiated readers of its historical value as the only, if fragmentary form, in which Cassiodorus's lost work on an important factor in history, has been preserved, and of the use made of it, as such, by medieval chronicles, and modern historians. Perhaps he would have emphasized this important point if he had made use of the two best treatments of his author, which are to be found in Wattenbach's *Deutschlands Geschichtsquellen im Mittelalter* and Manitius's *Geschichte der lateinischen Literatur des Mittelalters*.

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Thanks are due to Messrs. Lemcke & Buechner, 30–32 W. 27th St., New York, for material furnished.

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